Wm Whit

PHYSIOLOGICAL

ESSAYS

CONTAINING,

I. An INQUIRY into the Cauſes which promote the CIRCULATION of the Fluids in the very Small Veſſels of Animals.

II. OBSERVATIONS on the SENSIBILITY and IRRITABILITY of the Parts of Men and other Animals; occaſioned by M. DE HALLER's late Treatiſe on theſe Subjects.

The THIRD EDITION,

With an APPENDIX, containing an Anſwer to *M. de Haller*'s Remarks in the 4th Volume of the *Memoires ſur les parties ſenſibles et irritables.*

By ROBERT WHYTT, M. D. F. R. S.
Phyſician to his MAJESTY.
FELLOW of the ROYAL COLLEGE of PHYSICIANS, and PROFESSOR of MEDICINE in the UNIVERSITY of EDINBURGH.

EDINBURGH:
Printed for JOHN BALFOUR,
MDCCLXVI.

1766

CONTENTS.

I.

SECT. III.

II.

PART I.

PART II.

I.

AN INQUIRY

INTO THE

CAUSES which promote

The CIRCULATION of the FLUIDS

IN THE

Very SMALL VESSELS of ANIMALS.

The THIRD EDITION.

THE following Paper was read at ſeveral Meetings of the PHILOSOPHICAL SOCIETY *of EDINBURGH in the Years* 1745 *and* 1746; *and is now publiſhed with ſome Corrections and Additions.*

AN INQUIRY INTO THE CAUSES which promote the CIRCULATION of the FLUIDS IN THE *Very* SMALL VESSELS *of Animals*.

ALTHO' the circulation of the blood has been almoſt univerſally acknowledged for above a century paſt, and much has been written in order to explain this doctrine; yet there are ſeveral things relating to it which have not been, hitherto, accounted for in ſo ſatisfactory a manner, as to render any farther inquiry into them altogether ſuperfluous: and of this kind, we preſume, is the motion of the fluids in the ſmaller veſſels.

The

THE firſt authors who embraced the *Harvean* doctrine ſeem to have aſcribed the whole of the circulation, both in the arteries and veins, to the force of the heart *. But *Borelli*, in whoſe time it was believed by many, that the arteries and veins were not continued canals, but divided by an intermediate ſpungy ſubſtance, plainly ſaw, that, in this caſe, the blood could not be conveyed into the orifices of the naſcent veins, by any force of the arterial fluids pushing it forward; and, therefore he ſuppoſes it to enter them in the ſame manner as the particles of water inſinuate themſelves into a ſpunge or other porous ſubſtance: but as, in his days, the *phænomena* of capillary tubes were very little known, and the reaſons of them not at all underſtood, 'tis no wonder that,after declaring attraction to be an impoſſible thing, he aſcribes the above effects to the gravity of the fluid itſelf †; nor does he ſeem to have been ſufficiently aware, that, after water has riſen to a determinate

---

* Jo. Walaei epiſt. ad Bartholin. De motu chyli et ſanguinis.

† Borelli De mot. animal. pars 2. prop. 32.

terminate height in ſmall tubes, or a certain quantity of it has been received into porous bodies, no more of it will enter into either of theſe.

DR PITCAIRN, in his *Diſſertatio de Circulatione ſanguinis per vaſa minima*, after ſhewing that animal ſecretion cannot be performed by means of ferments in the glands, or by theſe bodies acting as filtres, endeavours to prove, that the various ſecretions from the blood are intirely owing to the different diameters of the ſecretory veſſels: but he makes it no part of his inquiry, by what *powers* the fluids are puſhed through theſe veſſels. However, that there might appear no difficulty in the motion of the fluids thro' even the ſmalleſt tubes of the body, nor any ſuſpicion of their ſtagnating in them, we have lately been told, that the blood moves more quickly in the ſmaller than in the larger veſſels; an aſſertion ſo inconſiſtent with the laws of hydraulics, when applied to the animal frame, that it could ſcarcely have been expected to have dropt from the pen of a writer much leſs noted than Dr *Hoffman* *.

---

* Frederic. Hoff. ſyſt. med. l. 1. § 1. c. vi. No. XVII.

But, how eaſy ſoever it may have appeared to ſome authors to account for the motion of the fluids in the ſmall veſſels of animals, yet whoever impartially conſiders the reſiſtance that a fluid, moving through the *aorta* and all its branches, muſt meet with from friction, which increaſes as the diameters of the veſſels decreaſe, and adds to this the mutual attraction and coheſion between the particles of the fluids and the ſides of the veſſels in which they move, will not only ſee that there is, at leaſt, ſome difficulty in this matter, but be alſo apt to ſuſpect that neither the force of the heart, nor the alternate contraction of the larger arteries, is ſufficient to drive the fluids through the ſmalleſt veſſels of the brain, *teſtes*, and many other parts of the body.

In order, however, to ſet this affair in a clearer light, we ſhall particularly conſider the ſeveral cauſes to which the circulation of the blood has been commonly aſcribed.

SECT. I.

Of the force of the heart, contraction of the arteries, gravity, and the attraction of capillary tubes, conſidered as cauſes of the circulation of the fluids in the ſmall veſſels of animals.

THE principal cauſe which propells the blood through the body, is, without doubt, the contraction of the heart: let us then, firſt, inquire how far this may be ſuppoſed ſufficient to account for the motion of the fluids in the very ſmall veſſels of animals.

IF the force with which the blood is thrown, by the left ventricle of the heart, into the *aorta*, be ſuppoſed equal to the preſſure of a column of blood 90 inches high*; the *momentum* of this fluid in any artery will be found, by multiplying the *area* of the tranſverſe ſection of that artery into 90, the height of that column of blood whoſe preſſure is ſuppoſed equal to the protruſive

---

* Dr *Hales*, from a variety of experiments made on horſes, dogs, ſheep, and other animals, thinks it probable, that the blood would riſe ſeven feet and an half, or 90 inches, in a tube fixed into the carotid artery of a middle-ſized man. Statical Eſſays, vol. 2. p. 40.

force of the heart: for the product gives the number of cubic inches or parts of a cubic inch of blood, whoſe weight is equal to the preſſing power with which the blood is driven by the force of the heart into that artery.

THE diameter of a circulating red globule of blood, has been generally reckoned ſomething leſs than $\frac{1}{3000}$ part of an inch; but Dr *Martine* has, from *Lewenhoeck*'s and *Jurin*'s later obſervations, ſhewn it to be $\frac{1}{1933.5}$ part of an inch *; and *Lewenhoeck* has obſerved, that one of theſe globules is ſometimes obliged, in paſſing through a very ſmall capillary artery, to change its figure into an oblong ſpheroid, ſo that the diameter of ſuch an artery may be ſuppoſed nearly equal to that of a red globule. If then, for the ſake of more eaſy computation, we ſuppoſe the diameter of a red capillary artery to be equal to $\frac{1}{2000}$ part of an inch, the *area* of its tranſverſe ſection will be 0.000 000 196, and this multiplied by 90 gives 0.000 0176 parts of a cubic inch of blood, which amounts to 0.00466 or $\frac{1}{214}$ part

---

* Edinburgh Medical Eſſays, vol. 2. art. vii.

part of a grain;* and is equal to the moment of the blood, ariſing from the preſſing force of the heart, in a capillary artery, whoſe diameter is $\frac{1}{2000}$ part of an inch, upon the ſuppoſition that there were no loſs of motion from friction, and that the areas of the tranſverſe ſections of all the capillary arteries in the human body were equal to that of the *aorta:* but ſince this is not the caſe, and the areas of the former greatly exceed that of the latter, the moment of the blood in a capillary red artery will fall very much ſhort of our computation.

To illuſtrate this; let us ſuppoſe a pipe A of an inch diameter, to be divided into ſeveral branches, and at laſt to terminate in 10000 ſmall tubes *a,a,a,a, &c.* each $\frac{1}{100}$ part of an inch in diameter; the ſum of the areas of whoſe tranſverſe ſections is equal to that of A. If a fluid be puſhed through ſuch a ſyſtem of veſſels, with any given force,

---

[*] A cubic inch of warm blood is reckoned by ſome 266, and by others a little more than 267 grains: but Dr *Martine* ſeems to have fixed it pretty accurately at $264\frac{3}{4}$; and, for the ſake of even numbers, I have ſuppoſed it to be 265 grains.

force, the velocities in the ſmall tubes *a,a,a,a, &c.* will be equal to the velocity in A; and their *momenta m,m,m,m, &c.* all taken together, will, bating friction, be juſt equal to the *momentum* M in the large trunk A, *i. e.* $m\ 10000=M$ or $m=\frac{M}{10000}$. But if another pipe B of the ſame diameter with A be divided ſo as to terminate in 300000 ſmall tubes *b,b,b,b, &c.* each $\frac{1}{100}$ part of an inch diameter; then, although a fluid be puſhed through the two trunks A and B with the ſame velocity, and conſequently the *momentum* in them be equal, yet the velocity in any one of the ſmall tubes *a,a,a, &c.* will be to the velocity in any one of the correſponding tubes *b,b,b, &c.* as 30 to 1, and conſequently their *momenta* will be as 900 to 1.

Dr Keill, having, by meaſuring the arteries of the human body, fixed the proportions of the branches to their trunks after every diviſion, lays down a method for calculating in what degree the velocity of the blood in the different arteries is affected by the increaſe of the capacity of the veſſels through which it flows *: according to this

---

* Keill's Tentamen. med. phyſ. 2.

this computation, it will be found, that the velocity of the blood in an artery whoſe diameter is $\frac{1}{2000}$ part of an inch, ought to be to its velocity in the *aorta*, as 1 to 345; and conſequently the moment of the blood in ſuch an artery muſt be $345 \times 345 = 119025$ times leſs than we have computed it above, *i. e.* $= \frac{1}{214} \times 119024 = \frac{1}{25471350}$ part of a grain. And ſince a globule of red blood weighs nearly $\frac{1}{50000000}$ part of a grain *, it follows, that the moment or preſſing force of ſuch a globule in its capillary artery, ariſing from the impulſion of the heart, does not exceed twice its own weight.

But even this moment, however ſmall it may appear, muſt be diminiſhed by friction: the preciſe quantity of which, although it may perhaps be difficult, with any certainty, to determine; yet that it muſt be very conſiderable, will evidently appear from what follows.

1. If two pipes of equal lengths, whoſe diameters are $\frac{372}{1000}$ and $\frac{90}{1000}$ parts of an inch, be, one after another, ſcrewed into the ſide of a veſſel at the perpendicular di-

ſtance

---

* Edinburgh Medical Eſſays, vol. 2. art. vii. § xi.

ſtance of four feet from the top of the water, and laid parallel to the horizon, the large pipe will diſcharge 179, and the ſmall pipe $6\frac{1}{8}$ ounces of water, in half a minute. Hence the velocities of the water in theſe two pipes muſt have been as 1293 and 756; and, were it not for the inquality of the reſiſtance of the air, the velocity in the large pipe would have been ſtill greater, and the velocities in the two pipes pretty nearly as the ſquare-roots of their reſpective diameters*.

Hence, if we could ſuppoſe a capillary artery, of $\frac{1}{2000}$ part of an inch diameter, to go off directly from the beginning of the *aorta*, without any intermediate branchings, the velocity of the blood in it would be (*cæteris paribus*) to the velocity of the blood in the *aorta*, nearly as $\sqrt[2]{0\cdot0005}$ the diameter of the capillary is to $\sqrt[2]{07}$ the diameter of the *aorta*, *i. e.* as 1 to 37·4; and conſequently the moment of a ſingle globule in ſuch a capillary artery would be to its moment in the *aorta*, as 1 to 1398.

2. But further, the loſs of motion from friction depends not only upon the ſmallneſs

---

* Robinſon's animal œconomy, prop. 1. exp. 2.

neſs of the veſſels, but alſo upon their diſtance from the heart: for, if two cylindrical pipes, whoſe common diameter is $\frac{345}{1000}$ parts of an inch, and whoſe lengths are 2 and 8 feet, be ſcrewed into the ſide of a veſſel full of water, at the diſtance of four feet from the top; the quantities diſcharged in half a minute, will be 97½ ounces by the long pipe, and 175 ounces by the ſhort one. Hence the velocities of the water in the two pipes were as 97½ and 175; ſo that, by the greater quantity of friction in the longeſt pipe, the water loſt above ⅖ of its velocity*.

3. Again, the velocity of the blood will be different according to the different angles at which the branches go off from their trunks; and the various flexures and convolutions of the ſmall arterial ramifications muſt increaſe the friction in them, and conſequently retard the motion of the blood conſiderably. This ſeems to be confirmed by an experiment of Dr *Hales*; from which it appears, that the velocity of the blood in the ſmall arteries decreaſes in a greater proportion than it ought to do by

---

* Robinſon's animal œconomy, prop. 1. exp. 1.

the above mentioned experiments made with ſtreight cylindrical pipes; for, having ſlit up the inteſtines of a dog from one end to the other, on the ſide oppoſite to that where the blood veſſels enter them, and fixed a braſs tube into the deſcending *aorta*, he found that, with a preſſure equal to the force of the heart, only $\frac{1}{3}$ of the water paſſed in a given time through the ſlit arteries of the guts that flowed through the meſenterics when cut over juſt at their entry into the inteſtines; notwithſtanding that the area of the orifices of all the former exceeded that of the latter, and that the diameters of the cut meſenterics did not exceed four times the diameters of the converging ſlit arteries of the guts *.

From what has been ſaid it may appear, that the velocity of the blood will not be the ſame in all the arteries of the ſame diameter, (as ſome have fondly imagined, and been at no ſmall pains to prove), but will be greater or leſs, according to their diſtance from the heart, the exceſs of the areas of the branches above their trunks, the

---

* *Hales*'s Statical eſſays, vol. 2. exp. ix.

the angles at which they go off, and the number and degree of their flexures.

AGREEABLE to this, Dr *Hales* obſerved, that, in a capillary artery of the lungs of a frog (where the diſtance from the heart is but ſmall, and where the exceſs of the area of all the branches above their trunk, is not near ſo great as in the other parts of the body), the blood moved forty three times faſter than in a capillary artery of one of the muſcles of the *abdomen* *: and it is probable that, next to the lungs, the blood moves quickeſt thro' the veſſels of the heart. In conſequence of this quick circulation, it muſt be evident, whether we ſuppoſe animal heat to ariſe from the friction of the blood on the ſides of the veſſels, or from an inteſtine motion among its ſmall particles, that, *cæteris paribus*, more heat muſt be generated in the lungs and heart than any where elſe; and hence the neceſſity of continual ſupplies of freſh air to cool the blood in its paſſage through the pulmonary veſſels. Nor is this opinion founded in theory alone; for, upon trial, it will appear, that the greateſt heat in

---

* Statical Eſſays, vol. 2. p. 68.

any animal is, almoſt always, about the heart. In a jackdaw, the heat below the wing made the mercury in my thermometer riſe to 104 degrees of *Farenheit*'s ſcale; within the *inteſtinum rectum*, it roſe to 107$\frac{1}{2}$; and, when applied to the heart, it reached 109. And agreeably to this, I have found the heat in a pigeon's heart above a degree greater than within the *inteſtinum rectum*.

Upon the whole, if the moment of a ſingle red globule of blood ariſing from the preſſing force of the heart, does not in its capillary artery, even bating friction, exceed twice its own weight or $\frac{1}{25471350}$ part of a grain; and if that loſs of motion which it muſt have ſuſtained by friction in its way from the heart thither be conſiderable, as one may reaſonably conclude from what has been advanced upon this head; it will follow, that the real remaining force of ſuch a globule, when it arrives at a red capillary artery, may probably fall ſhort of its own weight, and muſt be ſo extremely ſmall, that it can ſcarcely be ſuppoſed ſufficient to overcome

come the reſiſtance it muſt meet with, in paſſing through a veſſel by which it is cloſely embraced on all ſides, although the anterior fluid in the capillary veins were no obſtacle in its way.

I deſire it may be here underſtood that the above calculations are by no means intended as demonſtrations, but rather as illuſtrations, in the preſent argument concerning the force of the blood in the ſmaller veſſels; and, allowing that by them the moment of a red globule in its capillary artery comes out too ſmall, either from our having, with Dr *Hales*, rated the force of the left ventricle of the heart too low, or, with Dr *Keil*, the number of branchings of the arteries, and the proportion they bear to their trunks, too high; yet it muſt be evident, that the force of the heart muſt be inſufficient to puſh the fluids thro' all the inferior orders of veſſels; or, which is the ſame thing, that the left ventricle of the heart does not, by its direct projectile force at every contraction, puſh on and move forward the whole circulating fluids in all the veſſels of the body.

Dr Hales obſerved the blood's motion to be accelerated by every *ſyſtole* of the heart, not only in the ſmall arteries, but alſo in the naſcent capillary veins of the lungs of a frog *; and *Lewenhoeck* aſſures us he has ſeen the ſame thing in other parts of various animals: ſo that it is not to be doubted, that the projectile force of the heart reaches at leaſt as far as the capillary arteries of the firſt order, nay, is probably continued, for ſome ſmall way, along their correſponding veins; eſpecially when theſe are not far from the heart.

But that the moment of the blood in the red capillary arteries, at any conſiderable diſtance from the heart, muſt be very ſmall, will appear from an obſervation of Dr *Hales;* according to which the velocity of the blood in one of theſe arteries in the *abdomen* of a frog, was near 900 times leſs than the equable velocity of this fluid in the *aorta* of a man †; and conſequently 2.6 times leſs than we have computed it to be in a human red capillary: wherefore the exceſs of the moment

---

* Statical Eſſays, vol. 2. p. 69.

† Statical Eſſays, p. 47. and 68.

ment of a red globule, in ſuch an artery of a frog, above the reſiſtance it had to overcome, only amounted to $\frac{1}{173340000}$ part of a grain, and ſo muſt have fallen a good deal ſhort of $\frac{1}{3}$ of its own weight; ſuppoſing the globules of red blood in a man and a frog to be of the ſame magnitude, which does not ſeem improbable *.

If then the remaining moment of a red globule in its capillary artery, after having overcome the reſiſtance of the anterior blood in its correſponding vein, does not amount to $\frac{1}{3}$ of its own weight; it muſt be evident, that the ſerous and ſmaller globules which move along with the red ones muſt be applied, by the projectile force of the heart, to the orifices of the lateral ſerous arteries with a very inconſiderable force: ſuch a one, ſurely, as will be far from being able to puſh theſe fluids through the ſerous, lymphatic, and, for any thing we know, many more inferior orders of veſſels.

But, to ſet this matter in a ſtill ſtronger light, we ſhall, upon the principles above laid down, endeavour to inveſtigate the force of the heart at the origin of the nerves.

---

* Med. Eſſays, vol. 2. art. vii. § v.

Lewenhoeck tells us, that he diſcovered veſſels in the cortical part of the brain, which could not admit a globule whoſe diameter was $\frac{1}{128000}$ part of an inch *; and he obſerved the fibres of its medullary ſubſtance to be either quadrangular or hexangular: whence he concludes, that they muſt be compoſed of ſmaller fibres, whoſe extreme minuteneſs made it impoſſible for him to diſcover any thing of their figure, nor does he think they can ever be ſeen diſtinctly by human eyes †.

Dr Porterfield has, indeed, from an experiment of Dr *Hook*, computed the diameter of a ſingle nervous fibre to be $\frac{1}{21000}$ part of an inch ‡: but, as the beſt microſcopes have never been able to diſcover any cavities in the nerves, 'tis certain, that, if they are hollow tubes at all, the diameter of their cavities muſt be a great deal leſs than this, and perhaps fall ſhort of $\frac{1}{200000}$ part of an inch; for a microſcope, which magnifies the diameter of an object 800 times, would, upon this ſuppoſition, make the cavities of the nerves appear equal to a

point

---

* De cerebro, p. 35. † Epiſt. 34.

‡ Edin. Med. Eſſ. vol. iv.

point whoſe diameter is $\frac{1}{250}$ part of an inch, which is an object that may be diſcovered by a good eye. *Lewenhoeck*, 'tis true, towards the end of his days, and when turned of eighty years, pretended oftener than once to have ſeen cavities in the nerves very diſtinctly. But it happens unluckily for this diſcovery, that no body has been able to confirm it ſince his death: nor could he, when alive, though he ſaw theſe cavities himſelf, ſhew them to any one elſe; as appears from the following paſſage in his 32d epiſtle: *Id unum in hoc negotio male me habet, quod cavitates illas nemini poſſum conſpicuas exhibere; nam ſimulac illas oculis meis examinandas admoveo, ilico et minuta citius per exſiccationem conſidunt.* But if the ultimate fibres of the *medulla oblongata* were ſo fine that he could diſcover nothing of their ſhape or figure, as he himſelf confeſſes, it will not be thought probable that he could diſcover the cavities of the nerves, which ſeem to be a production of theſe, and at leaſt equally ſubtile with them.

But, leſt any one unaccuſtomed to ſpeculations of this kind ſhould think the motion

motion of a fluid through ſuch vaſtly ſubtile veſſels as the nerves almoſt impoſſible, let him reflect a little on the infinite diviſibility of matter, and particularly on the extreme ductility of gold, which may be drawn over ſilver ſo as the thickneſs of the ſkin of gold (in which however the beſt microſcope cannot diſcover the ſmalleſt pore) ſhall not amount to $\frac{1}{12000000}$ part of an inch*; *i. e.* $\frac{1}{60}$ part of what we ſuppoſe the diameter of the cavity of a nerve may be: ſo that the particles of ſuch a leaf of gold ſwimming in a fluid might paſs more eaſily through the nerves, than a ſingle globule of red blood does through its capillary artery.

FURTHER, a ſoap-bubble, when managed after Sir *Iſaac Newton*'s method, exhibits, juſt before it breaks, a black ſpot upon its ſuperior part; the thickneſs of which, according to his theory of light and colours, ſcarcely exceeds $\frac{1}{3000000}$ part of an inch. Hence we ſee, that a fluid compoſed of ſoap and water may be divided, by human art, into parts whoſe diameter is fifteen times leſs than that which we have aſſigned

---

* Memoires de l'Acad. des Sciences, an. 1713.

ed to the nerves; and conſequently that ſuch a compound fluid might eaſily paſs through their cavities.

Let us then ſuppoſe the diameter of the cavity of a nerve to amount to $\frac{1}{200000}$ part of an inch, and the area of its tranſverſe ſection will be 0.0000000000196; which multiplied into 90 (the height of a column of blood whoſe weight is ſuppoſed equal to the preſſing force of the left ventricle of the heart) gives 0.00000000176 parts of a cubic inch of blood, or $\frac{1}{1240000}$ part of a grain; which would be equal to the moment of the animal ſpirits at the origin of the nerves, ariſing from the impulſive force of the heart, if there were no loſs of motion from friction, and if the area of the tranſverſe ſection of the *aorta* were equal to the area of the tranſverſe ſections of all the extreme capillary veſſels, in which the numerous branches and ramifications derived from the *aorta* at laſt terminate. But, if we conſider how greatly the latter muſt exceed the former, and, upon Dr *Keill*'s principles, enter into a computation of the effect which this muſt have

have upon the motion of the nervous fluid; we ſhall find, that its velocity will be to that of the blood in the *aorta* nearly as 1 to 20000; and conſequently the moment of the nervous fluid, ariſing from the protruſive force of the heart, will be only equal to $\frac{1}{2140000} \times \frac{1}{400000000}$ $= \frac{1}{856000000000000}$ of a grain.

If we imagine a ſphere to be compoſed of the particles of the nervous fluid, whoſe diameter is equal to the diameter which we have aſſigned to the cavity of a nerve; then, taking its ſpecific gravity to be the ſame with that of water, its weight will amount to $\frac{1}{45228780325614}$ part of a grain, *i. e.* near 19 times more than the force with which it is puſhed forward by the contraction of the left ventricle of the heart, even upon the ſuppoſition that it had met with no reſiſtance from friction in its paſſage through the ſmall veſſels of the brain. Hence the *momentum* of a ſmall ſphere of animal ſpirits in a nerve, is 38 times leſs in proportion to its weight, than the moving force of a globule of red blood in its capillary artery. And the difference of

of their forces will be ſtill greater, in proportion to the reſiſtance which each has to overcome; ſince the reſiſtance to the motion of a fluid, from friction, muſt be, *cæteris paribus*, as much greater in the nerves than in the red capillary arteries, as the diameter of the latter exceeds the diameter of the former.

But further, ſince the longer any capillary is, the more will the motion of a fluid be retarded, and conſequently its force be diminiſhed in it; 'tis eaſy to ſee that in the nerves, whoſe cavities are ſo inconceivably ſmall, but whoſe length is generally very conſiderable, the force of the heart, which we have ſhewn to be ſurpriſingly little, muſt be altogether unable to overcome the friction, nay even the mutual attraction of coheſion betwixt them and their fluid, and, conſequently, be of itſelf, and when unaſſiſted by any other power, wholly inſufficient to propell the animal ſpirits to all the different parts of the body. And this, even upon the ſuppoſition that the nerves were continued directly from the extremely minute capillary ar-

teries: but, if we conſider how much the force of the blood muſt be broken in paſſing through the infinitely convoluted and amazingly fine veſſels of the cortical part of the brain, together with the follicles in which theſe are imagined, by ſome, to terminate; what we have been contending for, will appear ſtill more evident.

LASTLY, the above reaſoning receives additional weight from thoſe experiments which ſhew that the brain may be nouriſhed, perform its office, and afford ſufficient ſupply of ſpirits for carrying on all the vital and animal functions, although the blood is puſhed by the heart into its veſſels with a great deal leſs force than uſual. Thus the illuſtrious Baron *Van Swieten* informs us, that he tied both the carotid arteries of a dog without any obſervable harm to him; on the contrary, he continued twelve days healthful and lively: after which time he opened his ſkull, but could diſcover nothing praeternatural in the brain*. Now, as in this dog the brain could only be ſupplied by the vertebral arteries which inoſculate with the carotids, the velocity, and conſequently

---

* Comment. in Boerh. aphor. vol. 1. p. 266.

conſequently the moment of the blood muſt, at the ſame time that it was conſiderably leſſened in the ramifications of the former, have been ſo remarkably diminiſhed in thoſe of the latter, by reaſon of the ſmallneſs of the branches with which they communicate, compared with the trunks of the carotids, as to ſhew beyond doubt, that the ſecretion of the nervous fluid, and its derivation to the ſeveral parts of the body, do not depend ſo much upon the force of the heart as has been generally imagined, but muſt be, in a great meaſure, owing to ſome other cauſe.

HAVING ſhewn how inconſiderable the moment of the fluids ariſing from the projectile force of the heart muſt be, in the inferior orders of veſſels, and particularly at the origin of the nerves; we come now to take a view, ſomewhat different, of the matter, and to compare the real force of the left ventricle of the heart with the obſtacles it has to overcome, upon the ſuppoſition that at each *ſyſtole* it puſhes forward the whole circulating fluids in all the arteries and veins of the body.

Borelli computed the reſiſtance which the blood meets with in circulating through all veſſels of the human body, to be equal to 180000 pounds weight*: but though this be over-rating the matter very much, yet, after all the abatements that can be reaſonably allowed, there will remain a reſiſtance by much too great to be overcome by the force alone of the left ventricle of the heart; a force which cannot, in man, amount to above 60 pounds weight†, as far as can be gathered from the lateſt and beſt experiments which have been made on other animals, in order to determine the preſſing power of their heart. Yet, inconſiderable as this force is, it is not to be regarded as that which is communicated to the blood in the *aorta*, but only as the preſſure or weight ſuſtained by the whole internal ſurface of the left ventricle of the heart juſt when it begins to contract; and the force with which the blood is impelled into the *aorta*, will (ſince fluids preſs equally *undequaque*) bear no greater proportion to this, than the *area* of the orifice of the

---

* De motu animal. part. 2. prop. 73.

† Dr *Hales* makes it only 51 pounds, *Statical Eſſays*, vol 2. p. 40.

the *aorta* does to the whole internal ſurface of the left ventricle of the heart; *i. e.* ſuppoſing the area of the orifice of the *aorta* =0. 5 of a ſquare inch, and the internal ſurface of the left ventricle=15 ſquare inches*, as 1 to 30; and therefore the force with which the blood is puſhed into the *aorta* muſt fall ſhort of $\frac{1}{3}$ of 60 pounds weight. Hence a reſiſtance in the *aorta* equal to two pounds, will require a force of above 60 pounds exerted by the whole internal ſurface of the left ventricle of the heart to overcome it: from which it follows, either that the reſiſtance to the motion of the blood in the *aorta* and all its branches and ramifications muſt be leſs than two pounds, which I believe no body will affirm; or elſe that the protruſive force of the left ventricle of the heart alone, is unable to drive the blood through all theſe veſſels, and conſequently inſufficient, without the aſſiſtance of ſome other power, to carry on the circulation.

If any one ſhould, on this occaſion, have recourſe, with the learned *Borelli*, to the *vis percuſſionis*, we need only obſerve, that the

---

* *Hales*, loc. cit.

the force of the heart is evidently not a percuſſive, but a preſſing one; ſo that, although the leaſt percuſſive force may be greater than any finite quieſcent reſiſtance, yet this will not hold true of a preſſing force, which, in order to have any ſenſible effect, muſt be greater than the reſiſtance it has to overcome: to ſay otherwiſe, is to affirm that, with the preſſing force of one's hand, the greateſt mountain might be moved out of its place.

NOR is Dr *Keill's* account of this matter more ſatisfactory, *viz.* that, the blood being once put in motion, a very ſmall force in the heart may be ſufficient to keep it always in this ſtate: for this force muſt be equal to the loſs of motion ſuſtained by the blood in every circulation, and conſequently to the reſiſtance which this fluid meets with in its paſſage thro' all the veſſels of the human body; a reſiſtance by far too great to be balanced by the few ounces to which the *Doctor* has reduced the force of the left ventricle of the heart*.

BUT that the foundation upon which Dr *Keill* proceeds is falſe, and that the heart can

---

* Tentam. med. phyſ. 3 de vi cordis.

can really communicate a new motion to the blood when the old one is in a great meaſure loſt, and after all the fluids have been for ſome time almoſt entirely at a ſtand, is evident from the recovery of people who have lain for ſome time in a *ſyncope*, and from the revival of the ſleeping animals, which are, in appearance, dead all the winter-ſeaſon. But further, ſince the blood, when it returns to the right ventricle of the heart, has ſcarce $\frac{1}{10}$ of the force with which it was thrown into the *aorta**, 'tis plain that it acquires, every circulation, $\frac{9}{10}$ of its force in paſſing through the heart and lungs.

THUS much being ſaid to ſhew that the force of the heart is, of itſelf, not ſufficient to carry on the circulation, we ſhall next briefly conſider the alternate contraction of the *aorta* and its branches, which has been juſtly reckoned among the chief cauſes of the motion of the blood.

THE blood thrown out at every *ſyſtole* by the left ventricle of the heart, is not inſtantly tranſmitted through the capillary arteries into their correſponding veins, but

---

* *Hales*'s Statical Eſſays, vol. 2.

the greateſt part of it is accumulated in the now-dilated arteries, and is, during their ſucceeding contraction, conveyed on thro' the ſmaller veſſels. This contraction however of the arteries may, perhaps, be conſidered, rather as a continuation of the heart's force, than as any new power impreſſed on or communicated to the blood; ſince it does not appear that the arteries contract with a greater force than that by which they were dilated. But, whatever may be the force with which the *aorta* and its branches reſtore themſelves, we know certainly that it is leſs than the ſyſtolic power of the left ventricle of the heart; becauſe the blood is obſerved always to be projected to a greater diſtance from a cut artery during its *diaſtole*, than in the time of its *ſyſtole*. Whence it follows, that, if the force of the heart is inſufficient to account for the motion of the fluids through the inferior orders of veſſels, the alternate contraction of the muſcular coat of the *aorta* and its branches muſt be ſo likewiſe. It is, however, to be obſerved, that the ſanguiferous arteries, whoſe numerous branch-

es are diſperſed every where through the body, muſt not only, by their alternate contraction, contribute to puſh forward their contained fluids, but alſo, by their dilatation, ſo compreſs the inferior orders of veſſels, as ſomewhat to promote the motion of the fluids in them *. I ſhall only add on this head, that, as the alternate contraction of the arteries depends intirely upon their preceeding dilatation by the heart, ſo, in the ſerous and inferior orders of arterial veſſels, to which the projectile force of the heart ſeems not to reach, there is no ſuch alternate dilatation and contraction to be obſerved †.

WITH reſpect to gravity, which ſome have reckoned among the cauſes promoting the circulation, it is ſufficient to obſerve, that in a horizontal poſition of the body, it can have no effect; and, in an erect one, it muſt retard the return of the blood by the *vena cava inferior*, as much as it promotes its motion downwards in the *aorta* and its branches.

---

* Vid. Edinburgh Medical Eſſays, vol. 5. p. 2. edit. 3. p. 39. where this point is well illuſtrated by the ingenious Dr *Gilchriſt*.

† *Lewenhoeck*, epiſt. 65. p. 167.

There is ſcarcely any thing that will ſooner or more naturally ſtrike the mind of one who inquires into the cauſes of the motion of the fluids in the very minute veſſels of animals, as well as vegetables, than that ſurpriſing power of attracting liquors which capillary tubes are endowed with. But although the attractive power of capillary tubes may aſſiſt us in accounting for the imbibition of fluids by the veſſels commonly called abſorbents, as we ſhall afterwards have occaſion to ſhew; yet it muſt appear evident to every one acquainted with the *phænomena* of theſe tubes, that this attraction can be of no uſe in promoting the circulation of the blood in the capillary arteries and veins: ſince theſe veſſels are always full; or, if they were not, the fluids would be determined by it, equally backward towards the larger arteries as onwards to the veins.

SECT. II.

That the vibratory motion of the ſmall veſſels of animals is the principal cauſe promoting the circulation of their fluids.

HAVING ſhewn the inſufficiency of the powers already mentioned to account for the circulation of the fluids in the very ſmall veſſels of animals, we ſhall now proceed to explain what we imagine to be the principal cauſe of this circulation.

ALTHOUGH, as has been obſerved above, the regular alternate pulſation of the arteries does not extend beyond the capillaries of the firſt order, except, perhaps, in places very near the heart; yet we are not to conſider the ſerous, lymphatic, and other ſtill ſmaller veſſels, as unactive canals no ways contributing to promote the circulation of their different fluids: on the contrary, it ſeems highly probable, that theſe veſſels are continually agitated with very ſmall alternate contractions, to which the circulation in them is, in a great meaſure, owing.

MANY

MANY phyſiological writers have ſuppoſed an oſcillatory motion in the ſmall veſſels of animals *; but few have ſaid any thing ſatisfactory concerning the cauſe of this motion. *Baglivi* ſuppoſed the membranous parts of the body to derive their oſcillations from the *dura mater;* and the vaſcular ſyſtem and fleſhy fibres, theirs from the heart: but, as it is now paſt doubt that the *dura mater* has no other motion than what ariſes from the pulſation of its own veſſels or thoſe of the brain; and as the alternate contraction of the arteries, depending upon their preceeding dilatation by the blood thrown out by the heart, has no place in the ſerous, lymphatic, and inferior orders of veſſels; the vibratory motions of theſe canals muſt be deduced from ſome other cauſe.

MANY experiments and obſervations ſhew that the muſcular fibres of animals are ſo framed, as to be readily excited into

---

* Among others, the learned Dr *De Gorter*, in his treatiſe *De motu vitali*, has not only admitted a vital oſcillatory motion in the ſmall veſſels, but endeavours to ſhew, that, without this, the force of the heart would be unable to carry on the circulation, § lvi. *&c.*

to contraction by a *ſtimulus*. The ſmall veſſels, therefore, which are endowed with a muſcular coat, as well as the larger ones, muſt neceſſarily be agitated with alternate contractions, as often as they are acted upon by any thing capable of gently irritating them; but ſuch are the blood and finer fluids derived from it, which, while they ſlowly glide through the ſmall veſſels, ſtimulate their internal ſurface, ſo as to excite them into gentle but continually repeated contractions.

Some of the greateſt philoſophers and phyſicians, of ancient as well as later times, have imagined the blood to be a very active fluid, endowed with uncommon qualities, and, as it were, the fountain and ſource of life in animals *; nor do they ſeem to have been led into this opinion ſo much from any favourite theory, as from experiments and obſervations made on living and dying animals. But, without entering into, much leſs defending, the peculiar notions of thoſe authors concerning the blood, we ſhall only ſay, that this fluid is

---

* *Ariſtot.* Hiſtor. animal. lib. 3. cap. 19.; and *Harvey* De generatione animal. exercitat. li. lii. et lxxi.

is extremely well fitted to act as a gentle *ſtimulus* upon the ſenſible fibres of animals, whether we conſider its compoſition, heat, or inteſtine motion: for, while the ſaline and other acrid particles in the blood render it fit to irritate the tender veſſels, its heat and inteſtine motion keep all its parts in a perpetually vibrating ſtate, which muſt increaſe their ſtimulating power [*]. Agreeably to this, we find, that, in many inſects and ſome larger animals, the circulation becomes more languid as the weather grows colder, and, in the winter-ſeaſon, is altogether at a ſtand, till, by the heat of the returning ſpring, the particles of the fluids begin to be briſkly agitated, and conſequently the ſolids ſtimulated into contraction. Doctor *Harvey* has long ſince remarked, that the hearts of ſeveral ſhell-fiſhes are only ſeen to beat in warm weather [†]; and the curious obſervations of *Reaumur* have ſhewn us, that the lives of inſects may be lengthened or ſhortened, and

---

[*] See an Eſſay on the vital and other involuntary motions of animals, ſect. 3.

[†] De motu ſang. cap. xvii.

and made more or leſs active, by expoſing them to different degrees of heat and cold *.

THUS much being ſaid to ſhew, that the blood is well fitted to act as a *ſtimulus*, we ſhall offer ſome further conſiderations to prove, that the ſmall veſſels are, by its influence, really excited into alternate contractions. And,

1. WE are led to conclude this from what we obſerve in the larger canals and veſſels of animals. Thus the ſeveral portions of the inteſtinal tube are ſolicited into alternate contractions by the aliment, air, and bile, ſtretching their coats and ſtimulating their internal ſurface: and, as we imagine an alternate motion in the ſmall veſſels neceſſary to promote the circulation of the fluids in them, ſo we know certainly, that the periſtaltic motion of the guts is the principal cauſe which conveys the digeſted aliment down towards the *anus*.

NOT only the auricles and ventricles of the heart, but alſo the trunks of the *venæ cavæ* adjoining to the right *ſinus venoſus*, are continually agitated with alternate

---

* Hiſtoire des inſectes, tome 2. memoire 1.

contractions *. The trunks of the *vena cava* preſerve this motion, in animals newly dead, a conſiderable time after the pulſation of the heart has ceaſed; but no ſooner is the blood contained in theſe veſſels evacuated, and all new ſupplies intercepted by ligatures, than their ſides collapſe, and remain without the ſmalleſt motion †: whence we are led to conclude, that the alternate contractions of theſe veins are, like thoſe of the heart, owing to the blood acting upon them as a *ſtimulus*.

It is generally allowed by phyſiologiſts, that the *ſyſtole* of the larger ſanguiferous arteries, in which a remarkable pulſation obtains, is owing, not only to their elaſticity, whereby they endeavour ſimply to recover themſelves, but partly alſo to a proper muſcular contraction of their tendineo-carnous coat ‡: and, as this is excited by the blood puſhed into them by the heart, which, at the ſame time that it diſtracts their fibres, gently irritates their internal ſurface;

---

* Eſſay on vital motions, *&c.* p. 97. and 354.

† Bartholin. epiſt. cent. iv. p. 109. *&c.*

‡ The diminution of the ſtrength of the pulſe in an arm that is quite palſied, is a ſtrong proof that the larger arteries act partly by a muſcular power.

ſurface; it ſeems highly reaſonable to allow, that the ſmaller veſſels, endowed at leaſt with equal ſenſibility, muſt be excited into feeble but continually repeated contractions, by the gentle *ſtimulus* of their circulating fluids.

FURTHER, as there are ſome of the more imperfect animals which have no heart, the circulation in them muſt be owing to the contractile power of the veſſels themſelves excited into action by the *ſtimulus* of the fluids. And that the veſſels of thoſe animals, which, in a natural ſtate, have a heart, are endowed with a ſimilar power, ſeems proved by examples of monſters wanting a heart or any thing analogous to it *, in whom the fluids muſt have circulated chiefly by the power of the veſſels.

2. A variety of facts might be mentioned, which clearly demonſtrate an alternate contractile power in the ſmall veſſels of animals, and that this is exerted more or leſs according to the degree of irritation affecting them.

THUS,

---

* Vid. Van Swieten Comment. in Boerhaave Aph. vol. 1. p. 256.; and Hiſtoire de l'acad. des ſciences, 1703; & Memoires, 1740.

“ Thus, the ſteams of warm ſpirit of wine received into the eyes, not only cauſe a greater flow of tears from their veſſels, but, in a few ſeconds, produce an artificial inflammation in them, that is, they make the globules of red blood enter the ſerous or lymphatic veſſels of the *conjunctiva*. Now, as this additional moment of the blood, whereby it is enabled to dilate theſe veſſels, cannot proceed from the heart or larger arteries, ſince their force is not, nor can be altered in the preſent caſe; it muſt be owing to the extraordinary alternate motion excited in the veſſels of the eye by the ſteams of the ſpirit of wine.

“ I preſume it will not be alledged, that the vapour of ſpirit of wine raiſes an inflammation in the eye, by conſtringing its veſſels ſo as to occaſion an obſtruction in them, and that this obſtruction afterwards produces the inflammation, by leſſening the number of veſſels through which the blood paſſes, and conſequently increaſing its force upon the obſtructed ones: for, not to inſiſt on what might be eaſily proved, that no obſtruction can ever produce an

an inflammation except in ſo far as it gives riſe to an unuſual irritation; the ſpirit of wine ſhould, by contracting the ſerous and lymphatic veſſels of the *conjunctiva*, enable them to ſuſtain this additional force.

But further, why does tepid milk and water, or a poultice of bread and milk, leſſen an inflammation of the eye, while acrid aſtringent and ſpirituous things increaſe it? According to the doctrine of inflammation from mere obſtruction, together with an increaſed force of the heart and larger arteries, one would think that the former ſhould, by relaxing the ſmall veſſels, expoſe them to be ſtill more and more dilated by the increaſed force of the blood, and ſo increaſe the inflammation; while the latter ſhould, by contracting thoſe veſſels, enable them not only to reſiſt the blood impelled by the heart, but alſo to expell the obſtructing red globules. But the truth of the matter is, that the tepid milk and water and poultice, by relaxing the veſſels, leſſen or remove the irritation and ſenſe of pain, which, by raiſing uncommon contractions in the ſmall veſſels,

was the cauſe of the inflammation; while acrid aſtringent and ſpirituous applications, though they tend to contract the veſſels, yet, by increaſing their vibratory motions, greatly augment the force of the blood in them, and therefore muſt neceſſarily increaſe the inflammation.

β The heat, redneſs and inflammation, brought on the ſkin by bliſters and ſinapiſms, are not owing to any increaſe of the heart's force, or of the moment of the blood in the larger veſſels, though this is often an effect of their application; but merely to the action of thoſe irritating ſubſtances on the cutaneous veſſels, whereby the motion of the fluids in them is greatly augmented.

γ The ſudden redneſs and glowing warmth of the face, which, in women eſpecially, accompanies a conſciouſneſs of ſhame, and is commonly diſtinguiſhed by the name of *bluſhing*, can only be ſatisfactorily accounted for, from an increaſed motion of the ſmall veſſels of the face *.

The

---

* See an Eſſay on the vital and other involuntary motions of animals, p. 101. and 102.

THE extraordinary flow of ſpittle which happens to hungry perſons from the ſight or even the remembrance of grateful food, and the profuſe ſecretion of urine which hyſterical people are frequently ſubject to, cannot be explained without having recourſe to an increaſed motion ſuddenly excited in the ſmall veſſels of the ſalivary glands and kidneys; and clearly ſhew that the quantity of ſpittle and urine ſeparated by theſe organs, does not depend ſo much upon the force with which the blood is determined into their veſſels by the heart, as upon the greater or leſſer vibratory motions of the ſecerning veſſels themſelves. And in the ſame manner, is it not reaſonable to believe, that the motion of the fluids in the ſmalleſt veſſels everywhere through the body, is as much, perhaps more, owing to their gentle alternate contractions, than to the force of the heart and larger arteries?

* THE ſecretion of tears, which is very little affected by the different forces with which the blood is impelled by the heart, is immediately increaſed in a very great

degree

degree by acrid applications to the eyes, or by certain paſſions of the mind.

In the firſt caſe, the greater ſecretion is owing to the acrid matter, which, by its irritation, raiſes an uncommon alternate motion in the lachrymal veſſels. Nor can it be with reaſon objected here, that acrid things applied to the eyes or received into the mouth, occaſion a greater flow of tears or ſpittle, not by raiſing any ſtronger motion in the ſmall veſſels of the lachrymal and ſalivary glands, but merely by conſtringing their excretory ducts, and ſo ſqueezing out the liquors contained in them; ſince the quantity of tears and ſpittle diſcharged in ſuch caſes ſhews, that not only the excretion but the ſecretion in theſe glands is greatly increaſed. And if an irritation of the *pelvis* of the kidney, or *ureter*, from a ſtone lodged there, often occaſions an uneaſy ſenſation in the extremity of the *urethra* *; is it not reaſonable to think, that, upon the application of ſtimulating things to the orifices of the lachrymal and ſalivary ducts, theſe will not be affected

---

* Van Swieten Comment. in Boerh. Aphor. vol. i. p. 301.; et Morton De pthiſi, lib. ii. c. 3.

fected alone, but the irritation will, in ſome degree, be communicated to the ſmall ſecretory veſſels of their reſpective glands, ſo as to excite in them ſtronger and more frequently repeated contractions, and conſequently increaſe their ſecretions?

The flow of tears which accompanies certain affections of the mind, is, like the greater ſecretion of ſpittle from the ſight of grateful food, and the heat and redneſs of the face from a conſciouſneſs of ſhame, owing to an unuſual vibratory motion excited in the lachrymal veſſels in conſequence of theſe affections, and not to any compreſſion which the lachrymal gland may ſuffer from ſome of the neighbouring muſcles, which are then brought into contraction; for no degree of alternate compreſſion applied to this gland remarkably increaſes the ſecretion of tears, unleſs its veſſels, or thoſe of the eye, are thereby irritated.

3. We have already ſeen, that an increaſed alternate motion in the ſmall veſſels occaſions a quicker flow of liquors through them: and the following caſe will ſhew, that,

that, when this motion is much diminiſhed or wholly ſuſpended, theſe veſſels collapſe, and the circulation in them either becomes very languid, or ceaſes altogether.

A boy betwixt four and five years of age was, on *Saturday* afternoon, ſuddenly ſeized with an apoplexy or abolition of ſenſe and voluntary motion. On *Sunday* morning, at nine o'clock, when I firſt ſaw him, his pulſe was full and quick, and his eyes had ſomething of a glazed look; but in the evening this was more remarkable. *Monday* a little before noon, he was ſtill alive, but his breathing was very laborious, and his pulſe ſmall and quick; at this time, his eyes were more ſhrivelled than they uſed to be in thoſe who have been ſeveral hours dead.

THIS glazed appearance of the eyes could not be owing to the diminution of the heart's force, ſince the pulſe was full and ſtrong for twenty-four hours after the diſeaſe came on: nor can the failure of the pulſe, afterwards, account for the eyes appearing more ſhrivelled than is uſual in perſons

perſons newly dead. But if the circulation of the fluids in the ſmall veſſels be chiefly owing to a vibratory motion in them, and if this muſt ceaſe when the influence of the nerves is intercepted; in this boy, whoſe brain, eſpecially its anterior part, was ſo remarkably obſtructed, the motion of the fluids in the very ſmall veſſels of the *cornea* and the ſecretion of the aqueous humour muſt have been greatly diminiſhed; and hence the dimneſs and ſhrivelling of the eyes *.

THE withering of a member that is palſied, or deprived of the nervous power, is to be accounted for in the ſame manner; and is a proof that the circulation of the fluids through the inferior orders of veſſels, is not more owing to the force of the heart, than to the action of theſe veſſels themſelves. This withering of a palſied member has made ſome imagine, that nutrition is performed by the nerves: but the *phænomenon*, we ſee, is eaſily accounted for

---

* Dr *Nuck* obſerved the ſecretion by the glands to be much diminiſhed, or intirely ſtopt, after their nerves were obſtructed or compreſſed. *Vid. Adenograph. curioſ.* p. 16.

for without this ſuppoſition; and there are good reaſons to think that the nerves are *ſolely* ſubſervient to motion and ſenſation.

4. ALTHO' the alternate contractions of the ſmaller veſſels, which we have been contending for, are not remarkable enough to be diſcerned in moſt animals; yet they may be clearly ſeen in the legs of a bug: in the ſmall veſſels of which an extraordinary vibration is diſcovered by the microſcope *.

5. *Laſtly*, THE vibratory, tho' inviſible motion of the ſmall veſſels, is greatly confirmed, by that irregular motion, backwards and forwards, of the globules of blood in the capillary arteries, formerly obſerved in dying animals by *Lewenhoeck*, and lately deſcribed more accurately by the learned *M. de Haller* †. As this motion continues after the heart is cut out of the body, it cannot be owing to any impulſe of the blood communicated from the larger arteries: and we have no reaſon to aſcribe it to any *peculiar* attraction between the globules of blood ‡, ſince this fluid, neither

---

* Baker on the microſcope, p. 130.

† Act. Gottingenſ. Vol. IV. p. 351.

‡ Ibid. p. 354.

neither when received into ſmall glaſs tubes, nor in any other experiment, ſhews ſuch a power.

THIS oſcillation, however, of the blood globules may be accounted for, from an irregular vibratory motion in the ſides of the ſmall veſſels. We ſee that in animals newly dead, the *vena cava* is excited into alternate contractions by the *ſtimulus* of the blood contained, and that the fibres of the muſcles, upon being expoſed to the air, or the action of other *ſtimuli*, are frequently agitated with a weak irregular and tremulous motion; it is therefore reaſonable to conclude, that the ſmall arteries, which are of a ſimilar nature with the *vena cava*, and whoſe power of motion in living animals ſhews them to be in ſome degree muſcular, may, after the circulation ceaſes, by the *ſtimulus* of the cold air, or of the globules of blood contained in them, continue to be excited into ſmall but irregular contractions, which, tho' not obſervable even by the microſcope, yet are diſcovered by their effects: for it is eaſy to ſee, that by the ſmalleſt contraction of the ſides of a capil-

lary artery, the contained blood will be put in motion, which will be renewed as often as ſuch contractions happen.

THIS is ſtill farther confirmed from its having been obſerved, that a few globules of blood extravaſated between the *laminæ* of the meſentery aſcended and deſcended irregularly, and were agitated with the ſame kind of oſcillatory motion as in the ſmall arteries *: for as often as by the leaſt agitation in the air, or other cauſe, the *laminæ* of the meſentery approached nearer to one another, the globules would fly from that place; and would return to it again as ſoon as they receded from each other, in much the ſame manner as water ſuſpended between two panes of glaſs is obſerved to aſcend or deſcend, juſt as theſe panes are brought nearer to, or removed farther from each other.

THE objection againſt the reality of a vibratory motion in the ſmall veſſels of animals, becauſe the microſcope ſhews no ſuch thing in moſt animals, is of no great weight; ſince it cannot be doubted, that the particles of all bodies, eſpecially fluids, are

---

* Vid. Act. Gotting. Vol. IV. p. 354.

are affected by heat with a perpetual oſcillatory motion; and yet, unleſs the heat be great, the eye, even aſſiſted by the beſt microſcopes, cannot diſcern any ſuch thing.

Further, ſince the microſcope only ſhews the circulation of the fluids in the red capillary arteries, but not in the ſerous, lymphatic, and many inferior orders of veſſels, can it be expected that any alternate vibratory motion ſhould be diſcovered in theſe veſſels? Or, is it reaſonable to deny an alternate motion to all veſſels or particles of matter which are too ſmall to fall under the notice of our ſenſes?

Altho' the branches of the vine were tranſparent, ſo that the motion of the ſap in its veſſels could be ſeen by the help of a good microſcope; yet it is very probable we ſhould not be able to diſcover any vibratory motion in them: and yet the force of the ſap in the bleeding ſeaſon ſhews, that, beſides attraction, there muſt be a real propelling power exerciſed by the veſſels of the vine *.

If the diameter of the *aorta* in its *diaſtole* does not exceed its diameter when con-

---

* Vid. *Hales*'s Statical Eſſays, vol. 1.

tracted above $\frac{1}{5}$ of a line, *i. e.* $\frac{1}{50}$ of its diameter *; and if the change of diameter, which happens in the red capillary arteries and inferior orders of veſſels from their vibratory contractions, be three times leſs in proportion to the magnitude of theſe veſſels than the difference of diameter in the *aorta*, ariſing from its alternate *diaſtole* and *ſyſtole*; then the difference between the greateſt and leaſt diameter of a capillary artery capable of receiving only one globule of red blood, when moſt dilate or contracted, will be equal to $\frac{1}{150}$ part of its diameter; *i. e.* ſuppoſing its diameter $\frac{1}{2000}$ of an inch, $=\frac{1}{300000}$ of an inch; and the ſpace deſcribed by each ſide of ſuch an artery, when it performs one of its ſmall vibratory contractions, will be only equal $\frac{1}{600000}$ part of an inch, which is greatly too ſmall to be diſcerned by the beſt microſcope.

HAVING thus endeavoured, by a variety of arguments, to ſhew, that the ſmall veſſels of animals are, thro' the gentle *ſtimulus* of the fluids, continually agitated with alternate contractions; we ſhall now, briefly, point

---

* Vid. Weitbrecht in Comment. Academ. Petropolitan. vol. vii. p. 314.

point out their uſe in carrying on the circulation. And it muſt appear evident to every one, that the inferior orders of veſſels will not only not retard the motion of the fluids, but greatly promote it; ſince every ſmall portion of them will, like a little heart, by its alternate contractions, puſh on its contained fluid. Nor ought theſe contractions, however weak and imperceptible, to be thought unable to produce this effect; ſince the motion of the fluids in the very ſmall veſſels is far from being rapid, and juſt ſuch as might be expected to ariſe from this cauſe. Dr *Hales* has obſerved, that, in a capillary red artery in one of the muſcles of the *abdomen* of a frog, the blood moved only an inch in a minute and a half *: and it is probable,

that,

---

* Statical Eſſays, vol. ii. p. 68. *Lewenhoeck* and the illuſtrious *Senac* have alſo obſerved the motion of the fluids to be very ſlow in the ſmall veſſels. On the other hand, *M. de Haller*, in his late treatiſe on the motion of the blood, tells us, that he has frequently ſeen this fluid moving faſter in a ſmall arterial branch in the meſentery of a frog, than in the trunk whence it took its riſe (*a*). But we cannot conclude from this, that the velocity of the blood is greater, in a natural ſtate,

(*a*) Act. Gotting. vol. iv. p. 294, 295, and 299.

that, in the fineſt ſecretory veſſels of the brain, the fluids may not move above a Pariſian line or $\frac{1}{12}$ of an inch in a minute, *i. e.* not twice as faſt as the *minute* hand of a ſmall-ſized watch.

IF it be objected, that, as the capillary arteries and veins are deſtitute of valves, their alternate contractions muſt puſh the fluids equally back towards the heart, as onwards to the larger veins: it may be ſufficient to anſwer, that the reſiſtance ariſing from the ſemilunar valves of the *aorta*, and from the force of the heart and larger arteries *a tergo*, being greater than that which oppoſes the tranſmiſſion of the fluids into the larger veins; the fluids acted upon by

---

ſtate, in the ſmall branches than in their trunks; for this is repugnant to the moſt certain obſervations, which ſhew, that the capacity of the branches always exceeds that of the trunks from which they proceed. All therefore that can be fairly deduced from *M. de Haller*'s obſervations is, that the blood may move as faſt or even faſter in ſome of the ſmall arterial branches than in the trunks from which they take their riſe, as often as thoſe branches are affected with an unuſual irritation, or the other branches proceeding from the ſame trunk are obſtructed or contracted by cold or other cauſes.

by the ſmall vibrating veſſels muſt neceſſarily be determined towards the latter. But further, why may not the alternate contractions of the ſmall veſſels, like the periſtaltic motion of the inteſtines, proceed in ſuch manner as to impel their fluids more remarkably onwards to the veins than backwards to the larger arteries?

Upon the whole, as we conceive the motion of the blood in the larger veſſels, and even capillaries of the firſt order, to be owing to the alternate *ſyſtole* of the heart and arteries; ſo in the ſerous, lymphatic, and ſtill ſmaller veſſels, where this force either reaches not at all, or is greatly diminiſhed, the circulation ſeems to be carried on, chiefly by the vibratory motions of theſe veſſels themſelves: and, the finer fluids being in this manner tranſmitted into the larger veins, the pulſation of the neighbouring arteries, action of the voluntary muſcles, and alternate compreſſion made upon all the contents of the *abdomen* and *thorax* by the motion of reſpiration, will promote their return to the heart along with the red blood in the *venæ cavæ*.

What we have ſaid of the circulation of the fluids in general, we would have underſtood alſo of their motion in the ſecretory pipes of the ſeveral glands. In thoſe glands, whoſe veſſels are moſt patulous, the ſecretion may be partly, and indeed principally, carried on by the force of the heart and larger arteries; a proof of which ſeems to be the bloody urine paſſed by ſuch as have weak kidneys, after violent exerciſe: but in other glands, whoſe ſtructure is finer, and particularly in the brain and *teſtes*, the motion of the fluids in the ſecretory and excretory veſſels ſeems to be much leſs owing to the force of the arterial blood *a tergo*, than to the gentle vibratory contractions of the veſſels themſelves.

With regard to the nerves, which are generally conſidered as the excretory ducts of the brain; it is probable, that the derivation of their fluid to the various parts of the body is not only owing to a gentle oſcillatory motion in them and their ſurrounding membranes, but alſo, in ſome degree, to their attraction as capillary tubes; for no ſooner can there be a waſte of this fluid

fluid at the extremity of any nerve, whether this happens from exhalation, alternate compreſſion of the neighbouring parts, or any other cauſe, than, by its attractive power, it will be filled again. In the other glands, however, whoſe excretory ducts, by their union, ſoon form pretty large canals, no ſuch attraction will have place.

1. FROM what has been ſaid, it may appear, that we are not to conſider the force of the heart and contraction of the larger arteries, as the ſole cauſes of the circulation of the fluids in animals. The whole vaſcular ſyſtem is endowed with a moving power, which is conſtantly excited into action by the *ſtimulus* of the circulating fluids; ſo that while the ſmall veſſels, by means of friction, deſtroy in part the moment of the juices, they, at the ſame time, communicate, by their gentle vibratory contractions, a new impulſe to them. Every part therefore of the vaſcular ſyſtem, as well as the heart and larger arteries, nay every ſection even of the ſmalleſt veſſel, is to be conceived as promoting the circulation of the fluids; that great work, upon which

which the life of the whole depends, and in carrying on which every part almoſt of the body is active.

2. If the motion of the fluids in the inferior orders of veſſels be not ſo much owing to the force of the heart and larger arteries as to the gentle alternate contractions of thoſe veſſels themſelves, we may eaſily ſee why frictions, warm, penetrating, and ſtimulating fomentations, and cataplaſms, *&c.* are often more ſucceſsful than internal medicines, in removing obſtructions in the ſerous, lymphatic, and other ſmall veſſels; ſince they not only contribute to attenuate the obſtructing matter, but greatly increaſe the oſcillatory motion of theſe veſſels. For the ſame reaſon it is, that the warm mineral waters, pumped with conſiderable force upon a part affected with the rheumatiſm or *ſciatica*, have effected a cure after other remedies had been uſed in vain.

Warm ſpirit of wine, either alone or mixed with other things, proves often a good deobſtruent when applied externally: yet I have known ſome people who were

were afraid to uſe it with this intention, becauſe it is known to coagulate the *ſerum* of the blood: but their fears were without foundation; for the quantity of ſpirit of wine which enters by the pores of the ſkin, is ſo ſmall as to be in no danger of producing any *coagulum;* beſides, as it is taken in by the abſorbent veins, it muſt go to the heart, and be mixed with the maſs of blood, before it can come at the obſtructed veſſels, unleſs when the obſtruction happens to be in any of thoſe glands in which the valvulous lymphatics terminate; for ſince my very ingenious Colleague Dr Monro *junior* has proved theſe lymphatics to be no more than abſorbent veins*, they muſt carry the finer parts of ſuch ſubſtances as are applied to their mouths, directly to thoſe glands which they enter, and before they can be mixed with the maſs of blood. But, altho' little is to be expected from the reſolving, or to be dreaded from the coagulating power of the ſpirit of wine, yet it proves, in many caſes, a good deobſtruent, by raiſing an uncommon

---

* Vid. Diſſertat. de venis lymphaticis valvuloſis.

mon oſcillatory motion and heat in the veſſels of the part to which it is applied.

3. IF the circulation in the ſmall veſſels be, in a great meaſure, owing to their vibratory motion excited by the *ſtimulus* of the circulating fluids, it will follow, that, when theſe veſſels, in any part of the body, are affected with an extraordinary irritation, they muſt neceſſarily be agitated with much ſtronger and more frequently repeated contractions than uſual: whence the force of the blood in them will be greatly increaſed; and the ſmall arteries will not only be, more than uſually, diſtended with blood, and conſequently the part inflated, but the red globules will be puſhed in to the ſerous veſſels,* and in many

---

* Although an inflammation *ab errore loci* may not happen ſo often as has been alledged, yet the inflammation of the *cornea* and *conjunctiva* covering it, whoſe veſſels in a natural ſtate do not admit red globules, is a ſufficient proof that inflammations have not only their ſeat in the red capillaries, but alſo in the ſerous arteries. Nay the effuſion of blood into the ſpaces of the *tela cellulaſa*, is itſelf a proof of an *error loci* in inflammations, ſince this effuſion is much ſeldomer owing to a rupture of the ſmall red arteries, than to a dilatation of the orifices of thoſe veſſels which in a natural ſtate only tranſmit a thin, colourleſs fluid.

many caſes will be forced into the ſpaces of the *tela celluloſa*, through the dilated orifices of the ſmall arteries, which terminate in them *; and this muſt happen, whether the force of the blood be, or be not increaſed in the other veſſels of the body. An inflammation, therefore, is not owing to an increaſed force of the heart and larger arteries conſequent upon an obſtruction, as ſome authors of great name have imagined, but to an increaſed alternate contraction in the ſmall veſſels, whether this ariſes from ſome obſtructing matter diſtracting their fibres, or acrid matter irritating them. An obſtruction without an irritation in the obſtructed part, never occaſions an inflammation; but the irritation of any ſenſible part with a ſharp inſtrument, or acrid matter, never fails to produce this effect, although there be no preceeding obſtruction, nor increaſe of the heart's force. When a large artery is tied in the operation of the aneuriſm, we don't find, that the increaſed moment of the blood in the neighbouring arteries produces an inflammation in the arm; but, when

---

* Haller. Element. phyſiolog. tom. 1. lib. 2. ſect. 30.

when a tendon is wounded in blood-letting, or a little acrid matter is collected below the nail, a remarkable pain, ſwelling, and inflammation follow.

However, altho' an increaſed force of the blood in the large veſſels is not the cauſe of an inflammation, yet it is frequently the conſequence of it: for, as often as the inflammation is large, or the part inflamed very ſenſible, the whole nervous ſyſtem will be ſo affected by the pain, as to render the heart and larger arteries more irritable, at the ſame time that the blood, now vitiated by the obſtruction and inflammation, muſt act upon them as a ſtronger *ſtimulus* than uſual. Hence we may ſee, why, in inflammations, the pulſe is often little changed till the diſeaſe has continued for ſome conſiderable time. In inflammations of the ſtomach, inteſtines, and *uterus*, the pulſe, though much quickened, often continues ſmall; becauſe, on account of the particular ſympathy between their nerves and thoſe of the heart, this muſcle is rendered ſo irritable, as to contract before its ventricles are filled with the returning venous blood.

FROM what has been ſaid it may appear, that, in the cure of inflammations, beſides diminiſhing the force of the circulation in general by blood-letting, a particular regard is to be had to the veſſels of the part affected, whoſe extraordinary alternate contractions ſhould be leſſened by proper emollient and anodyne applications, and, in many caſes, by bliſtering the neighbouring parts. My ingenious friend Dr *Pringle* has often obſerved the good effects of bliſters, even when early applied, in pleuriſies and other internal inflammations *. And I have frequently ſeen a bliſter, in fifteen hours, leſſen remarkably the quickneſs of the pulſe, not only in obſtructions of the lungs attended with a fever and conſiderable expectoration of phlegm †, but alſo in pleuritic caſes, and in an *angina*, after blooding once and again had done little this way; nay, in obſtructions of the lungs and pleuriſies I look on it as one of the worſt ſigns, when after proper bleeding a large bliſter does not leſſen the quickneſs of the pulſe;

---

* See his obſervations on the diſeaſes of the army, 1ſt edit. p. 173. 178. and 179.

† See Philoſophical tranſact. vol. L. part. ii. p. 569.

pulſe; for I have rarely ſeen any ſuch caſes that did not prove fatal. I know many phyſicians have entertained prejudices againſt bliſtering in inflammations, becauſe, by their irritation, they increaſe the force of the circulation in general: but, not to mention the good effects they may have by attenuating the obſtructing matter, and making a conſiderable derivation of ſerous humours from veſſels which are nearly connected with thoſe of the part affected; if the account we have given of inflammations be true, it muſt follow, that altho' the *material* cauſe of an inflammation, *i. e.* the acrid or obſtructing matter, be not immediately removed by bliſtering; yet if, according to *Hippocrates*'s obſervation *, the painful ſenſation in the inflamed veſſels be leſſened by its means, the extraordinary alternate motions of theſe veſſels, and conſequently the cauſe continuing and increaſing the inflammation, muſt be alſo leſſened. Hence it appears, that a bliſter, though it

---

* *Δυο πόνων ἅμα γινομενων, μὴ κατα τὸν αὐτὸν τοπον, ὁ σφοδροτερως ἀμαυροῖ τὸν ἕτερον.* Duobus doloribus ſimul obortis, non in eodem loco, vehementior obſcurat alterum. Aphor. lib. 2. No. 46.

it tends to increaſe the force of the circulation in general, may yet leſſen the *impetus* of the blood upon the veſſels of an inflamed part more remarkably than even blood-letting itſelf. In patients in whom there is no fever or increaſe of the circulation from any inflammation, bliſters are obſerved, by their *ſtimulus*, to raiſe the pulſe and augment the heat of the body; but in internal inflammations, after ſuch bleeding as the circumſtances of the patient may require, bliſters often abate the fever and heat of the body, as well as the quickneſs of the pulſe, by leſſening or removing the inflammatory obſtruction.

WHAT has been ſaid of bliſtering, may be applied alſo to cupping and ſcarifying in pleuriſies, *angina's*, *&c.*

SINAPISMS, laid to the ſoles of the feet, remove or leſſen ravings, not by determining the blood more copiouſly to the inferior extremities, for their effect in this reſpect is altogether trifling; but by raiſing a very conſiderable pain, which ſo affects the mind, as to render it leſs ſenſible of the unuſual *ſtimulus* or irritation in the brain,

or its membranes, *i. e.* of the cauſe producing and continuing the *delirium.* Nor is it material to what part of the body thoſe cataplaſms are applied; for a ſtrong *delirium*, in a fever, has been removed by the application of a ſinapiſm, by miſtake, inſtead of a poultice of *theriac*, to the region of the ſtomach.

We may alſo, from what has been ſaid, ſee how ravings, phrenſies, and madneſs have been cured by the power of muſic *, or by a ſudden fright †; for theſe, by greatly affecting the mind, and fixing its attention, not only render it leſs ſenſible of the diſordered ſtate of the brain and its membranes, but, by the ſtrong impreſſion they make on the *ſenſorium commune*, may tend to diſlodge or remove the cauſe of the diſeaſe.

---

* Hiſtoire de l'acad. des ſciences, 1708 & 1717.

† Van Swieten Comment. in Boerhaave Aph. § 11.

SECT.

SECT. III.

Of the motion of the fluids in thoſe veſſels of animals commonly called abſorbent.

BESIDES the ſmall veins, which are continued veſſels with the arteries, and terminate at laſt in the two *venæ cavæ*, there are others which take their riſe from the internal ſurfaces of the ſeveral cavities in the body, and from the ſkin: and, as the fluids which theſe veſſels convey cannot be impelled into them by the force of the heart or arteries, they have been thought to receive them by ſuction, and therefore have got the name of abſorbent or imbibing veins. In the inteſtines we find two kinds of them, *viz.* the lacteal veins, and thoſe commonly called abſorbent; which laſt are alſo to be found upon the ſurface of the ſkin, *peritonæum*, *pericardium*, *pleura*, veſicles of the lungs, *dura* and *pia mater*, and, in ſhort, of every membrane which lines any cavity of the body. In accounting for the motion of the fluids in theſe veſſels, we ſhall begin with the lacteals; in order

order to which it may be neceſſary to premiſe,

1. THAT the lacteal veins have their origin in the villous coat of the guts, where their orifices are ſo ſmall as to eſcape the eyes of anatomiſts: that leaving the poſterior ſurface of the villous, they paſs thro' the nervous and muſcular coats, and, uniting into larger canals, are diſtributed in the form of a net-work in the external cellular membrane of the inteſtines; and that after this, they enter the meſentery, and are furniſhed with numerous valves, which hinder the return of any thing to the inteſtines.

2. AS often as the muſcular coat of the guts is contracted, the lacteal veins, which paſs between the interſtices of its fibres, and are diſtributed in the nervous and external cellular membranes, muſt neceſſarily be compreſſed; but are relaxed and freed from this preſſure, when this coat ceaſes to contract.

3. MANY and repeated experiments have ſhewn, That ſmall glaſs tubes are endowed with a power, by which they attract fluids,

ſo as to raiſe them conſiderably above the liquors in which they are immerſed---That this power increaſes exactly in the inverſe *ratio* of their diameters---That theſe tubes, whether ſtraight or crooked, in a perpendicular or oblique poſition, in *vacuo* or the open air, attract fluids to the ſame height, provided their diameters be equal.---That, when a capillary glaſs tube ends in a larger canal, the fluid is elevated ſo as to fill the capillary, but does not aſcend any further. ---That, if the diameter of a glaſs tube exceeds $\frac{1}{10}$ of an inch, its power of attraction is ſcarcely perceivable: and laſtly, That the ſame glaſs tubes attract different fluids to different heights, and this neither in proportion to their tenacity nor gravity. From all which it is natural to conclude, that the lacteal veins, which, in their beginning at leaſt, are ſmaller than any glaſs tubes made by human art, muſt be endowed with a remarkable power of attracting the chyle, when applied to their orifices.

How far the attractive power in ſuch canals as the lacteals and other abſorbent veins is, *cæteris paribus*, greater or leſs than

than in glaſs tubes, we have no experiments to determine: but, as the urine, an animal liquor, is more ſtrongly attracted by glaſs capillaries than water or any other fluid *; it is not unreaſonable to ſuppoſe that animal capillaries may be endowed with a ſtill ſtronger power of attracting it. And, as the ſame fluid is differently attracted by capillary tubes of different natures, tho' of the ſame diameter †; is it not probable, that the ſeveral abſorbent veins in animals may be peculiarly fitted to attract their proper liquors moſt ſtrongly?

Further, the remarkable attractive power with which the ſmall veſſels of vegetables are endowed, and by means of which they draw from the ſame moiſt earth very different juices, is a ſtrong argument for allowing a ſimilar attraction in the veſſels of animals. It is by this power that the ſap continues to riſe in the veſſels of trees, even in the cold ſeaſon of winter, tho' ſlowly and in ſmall quantity: nor can it

---

* Muſchenbroeck de tub. capill. vitr. cap. 3.

† Muſchenbroeck Element. philoſ. natural. cap. xviii. § 531.

it be pretended, that the ſun's heat promotes the aſcent of the ſap here, as it does in ſummer; ſince trees in cold cloudy weather, provided it be dry, and in places which the winter ſun-beams cannot reach, take up continually, by their roots, as much moiſture as is neceſſary to ſupply the waſte by perſpiration in their trunks and branches. But further, Dr *Hales* has obſerved, that cut branches will imbibe from the ſmall end immerſed in water to the great end, as well as from the great end immerſed in water to the ſmall end *: whence it clearly follows, that the aſcent of the ſap in the veſſels of plants, is not owing to any peculiar ſtructure in them, but ſolely to capillary attraction.

'Tis true indeed that capillary attraction, tho' it muſt make the ſap riſe in plants, will not, without the aſſiſtance of ſome other cauſe, make a continued derivation of it from their roots to their branches and leaves; becauſe as ſoon as capillary tubes are filled, or have raiſed fluids to a certain height, all motion from attraction ceaſes: but as the action of the air and ſun-beams upon

---

* Statical Eſſays, vol. 1.

upon the trunks, branches, and leaves of trees, occaſions a ſtrong perſpiration of the ſap by their pores; a proportional quantity will be attracted from the earth by their roots, to ſupply this waſte and keep the capillary veſſels always full. However, as often as the abſence of the ſun and the cool moiſt ſtate of the air put a ſtop to the perſpiration of vegetables, the ſap ceaſes to aſcend; nay, if the earth be warm and dry, the ſap gets a retrograde motion: and hence it is, that, in a cool ſummer's evening when the dew begins to fall, vegetables attract the watery particles in the air by the pores of their leaves and branches, in like manner as they had done the moiſture of the earth by their roots, in the day-time *.

THESE things being premiſed, it will be eaſy to account for the imbibition of the chyle by the lacteal veins.

WHEN any proportion of the inteſtines is relaxed, the lacteal veſſels, whoſe open mouths are every where to be found on the ſurface of the villous coat, take in the chyle by their attractive power, ſo as to fill their branches which are diſperſed in the nervous

---

* Vid. *Hales*'s Statical Eſſays, vol. 1.

nervous and external cellular membranes of the gut. The chyle being thus received into the capillary lacteals, is, by the ſucceeding contraction of the muſcular coat of the inteſtine, which compreſſes them, puſhed on towards the meſentery. As ſoon as this contraction ceaſes, the emptied lacteals, being free from compreſſion, fill themſelves with chyle as before, which the ſucceeding contraction of the gut preſſes forward to the larger lacteals in the meſentery. And thus we ſee the chyle is by turns attracted and propelled by the capillary power of the lacteals and periſtaltic motion of the inteſtines.

Further, it is probable, that the lacteal veins are, like the other ſmall veſſels of animals, agitated with a vibratory motion, excited in them by the gentle irritation of the chyle, which muſt aſſiſt the alternate contractions of the inteſtines in the propulſion of this fluid. Without allowing ſuch a vibratory motion in the umbilical veins of the chick, it will be no eaſy matter to account for its growth during the time of incubation. 'Tis true, the umbi-

lical arteries and veins run cloſe together in oviparous as well as viviparous animals, ſo that the alternate pulſations of the former muſt contribute to the propulſion of the fluids in the latter towards the heart. But, as there is no pulſation to be obſerved in the heart or umbilical arteries of the chick, till towards the end of the ſecond day *; and as, at any rate, the pulſation of the umbilical arteries does not extend beyond the red capillaries; the fluids in the extreme branches of the umbilical vein muſt owe their motion to ſome other cauſe. And is it not reaſonable to think that the colliquated white is conveyed thro' theſe veſſels by their attractive power, as capillary tubes, aſſiſted by the ſmall alternate contractions excited in them by the gentle *ſtimulus* of this warm fluid? And in this opinion we are confirmed by the analogy of plants; in whoſe veſſels the circulation of the ſap is greatly aſſiſted by a vibratory motion, which ſeems to be excited in them chiefly by the ſun's heat. And is not the remarkable force of the ſap in the bleeding vine owing to its veſſels being

---

* Malpigh. De ovo incubato.

being ſuſceptible of much ſtronger vibrations than thoſe of moſt other plants *?

The chyle in the larger lacteal veins which run along the meſentery, and are provided with valves, is puſhed on to *Pecquet*'s receptacle by the force of the new chyle continually tranſmitted to them from the guts, by the pulſation of the ſanguiferous arteries which run contiguous with them, and by the alternate motion of the diaphragm and abdominal muſcles in reſpiration.

If the chyle is received into the naſcent lacteal veins of the guts by their attraction as capillary tubes, it will be eaſy to ſee why quick-ſilver, which is repelled by ſuch tubes, ſhould, when ſwallowed by itſelf, generally paſs through the inteſtines without almoſt any of it getting into the blood. On the other hand, if the propulſion of the chyle is owing to the alternate contractions of the guts, it may eaſily appear, why it ceaſes to be tranſmitted thro' them

---

* *Dr. Hales* has obſerved, that, in a ſtem of a vine $\frac{7}{8}$ of an inch diameter, the force of the ſap in the bleeding ſeaſon was five times greater than the force of the blood in the crural artery of a horſe. Statical Eſſays, vol. I. exp. 36.

them ſoon after death; and why, in a well-fed animal newly killed, the lacteals in the meſentery, after being emptied, may be filled again, by gently preſſing the inteſtines and imitating their periſtaltic motion.

WITH reſpect to the abſorbent veins of the inteſtines; the finer parts of the digeſted aliment received into them by their attraction, are propelled towards the larger meſeraic veins and *vena portarum*, by the alternate contractions of the muſcular coat of the inteſtines and preſſure of the abdominal muſcles and diaphragm in reſpiration. But, as theſe abſorbents are not provided with valves, like the lacteals, it may be aſked, Why the laſt-mentioned power does not preſs the abſorbed fluids equally backward to the guts, as forward to the *vena portarum*? This we imagine is prevented,

1. BY the gentle alternate contractions of the abſorbent veins, which, as they are owing to the *ſtimulus* of the imbibed liquor, muſt begin at their orifices, and proceed towards their larger trunks. Such a motion as this, tho' gentle, will determine the

the courſe of the fluids on to the larger veins, but oppoſe their return to the inteſtines. And we find in fact, that, by means of a ſimilar motion in the inteſtines, the uſeleſs part of the aliment is conveyed to the great guts, even in a horizontal poſition of the body, where the alternate preſſure of the diaphragm and abdominal muſcles ought to puſh the contents of the bowels as much backwards to the ſtomach as forwards to the *colon*. But,

2. WHEN any portion of the inteſtines is contracted, the naſcent abſorbent veins, which riſe from the villous coat, and paſs thro' between the other membranes of this part, muſt have their ſides preſſed together, ſo as to allow nothing to paſs through them; wherefore the preſſing force of the muſcles of reſpiration muſt, if acting at this time upon the larger trunks of the abſorbent veins, propell their fluids towards the *vena portarum*. When this portion of the inteſtine is relaxed, the emptied abſorbents will, by their attraction, greedily fill themſelves with new fluids from its cavity: ſo that, whether the guts are contracted or

relaxed, there will be always ſome obſtacle to the retrograde motion of a fluid in the abſorbent veins.

When the liquors taken up by the capillary abſorbents are conveyed into the larger meſeraic veins, they will be carried along with *their* blood to the *vena portarum.*

As there are, upon the internal ſurfaces of all the cavities of the body, exhaling arteries which perpetually throw out a fine fluid to moiſten and lubricate the parts; ſo there are bibulous veins which take it up; whoſe exiſtence is proved, not only by no liquors being, in health, collected in theſe cavities, but alſo by anatomical injections *.

These abſorbent veins, which, like thoſe of the guts, have no valves, take up, by their attraction as capillary tubes, the rorid vapour of the arteries; after which it is conveyed either to the valvulous lymphatics † or to the ſanguiferous veins in which they terminate, by their vibrating motion, the pulſation of neighbouring arteries, and the alternate compreſſion of the muſcles. The abſorption in the cavities of the *abdomen*

* See Kauu perſpiratio Hippocrati dicta.

† Vid. Al. Monro jun. diſſertat. de ven. lymphat. valvuloſ.

men and *thorax* is greatly promoted by the alternate preſſure of the muſcles concerned in reſpiration; while the muſcles of voluntary motion employed in all kinds of exerciſe and labour, by accelerating the motion of the fluids in the abſorbent veſſels of the trunk and extremities of the body, enable them to imbibe more copiouſly. And hence we may ſee, why animals which move little, are generally oppreſſed with fat; while thoſe which are kept at hard labour, are very lean. In the former, the abſorbent veins of the fatty cells imbibe the oily matter depoſited there very ſlowly, becauſe they want the alternate preſſure of the muſcles of voluntary motion to puſh their contained fluid forward to the larger veins: In the latter, the abſorption from thoſe cells is not only increaſed by the various and continually repeated preſſure of the acting muſcles, but the body being, by much exerciſe, in ſome meaſure exhauſted of fluids, the veins imbibe more greedily, while the exhaling arteries pour forth their oily liquor more ſparingly.

If the exhalent veſſels of any cavity throw out too much, or if the abſorbent power

power of the veins be weakened, or if both theſe happen together, a watery fluid will be collected in it; and in this way are produced an *aſcites*, *hydrocele*, *hydrops pectoris*, *&c.*

When the blood is thin and watery and the veſſels weak, anaſarcous, œdematous, and other dropſical ſwellings are common: for, as the bibulous veins can, by their attraction, only take up fluids in proportion to the depletion they ſuffer by means of their own vibratory contractions, and the alternate compreſſion of neighbouring arteries and muſcles; their abſorbing power muſt neceſſarily be leſſened in a lax ſtate of the fibres, where thoſe cauſes are much weakened.

Further, while the redundance of a watery fluid in the blood increaſes the exhalation by the ſmall arteries, it leſſens the imbibition by the veins, for the ſame reaſon that aſhes, ſugar, or ſalts, when moiſtened, attract the watery particles of the air leſs ſtrongly than when they are dry.

Again, although there be little or no fault in the blood itſelf, yet, if its return from

from any part to the heart be much retarded, a dropſy of that part will ſoon follow; becauſe the fluids taken up by the abſorbents will be ſlowly and not without difficulty received into the larger ſanguiferous veins: and, as we have juſt now obſerved, their abſorption muſt be in proportion to their depletion. Hence we ſee, why ſchirrous tumors, ligatures, and whatever compreſſes the veins, ſoon bring on dropſical ſwellings.

It alſo appears from what has been ſaid, in what manner diuretics and purgatives carry off the ſtagnating waters in an *aſcites* and other dropſies: for by the diſcharges they make by the kidneys and inteſtines, they not only leſſen the quantity of watery fluid in the blood, but alſo, by their *ſtimulus*, increaſe the force of the circulation, whence the exhalation by the arteries muſt be leſſened, at the ſame time that the imbibition by the veins is increaſed.

The ſurface of the ſkin and veſicles of the lungs are, like the other ſurfaces in the body, endowed with exhaling arteries and abſorbent veins: by the former, there is perpe-

perpetually diſcharged from the blood a fine lymphatic fluid; and, by the latter, the watery particles floating in the air are conſtantly conveyed into it.

When the air is moiſt, and the body has been exhauſted by fatigue, the imbibition by thoſe veins often exceeds the exhalation by the arteries; as Drs *Keill* and *Linning* have obſerved *: but, taking the whole year round, the perſpiration made by the ſkin and lungs exceeds their imbibition by about forty ounces a-day in *Great Britain*, and

---

* Medicin. Stat. Britain. tab. iv. et obſervat.; et Philoſoph. Tranſact. No. 470.

The remarkable imbibition by the ſkin obſerved by Dr *Linning*, *July* 3. 1740, betwixt $2\frac{3}{4}$ and $5\frac{1}{8}$ afternoon, happened, 'tis true, without any preceeding fatigue; but is eaſily accounted for from his having, in that time, diſcharged $28\frac{6}{8}$ ounces of urine: ſince ſo great a waſte of the thinner parts of the blood muſt not only have diminiſhed the exhalation by the cutaneous perſpiring arteries, but alſo have increaſed the abſorbent power of the imbibing veins every where through the body: and hence it is, that in a *diabetes* the urine often not only exceeds the quantity of liquors drank, but theſe are taken up ſo greedily by the abſorbent veſſels of the ſtomach and inteſtines, as to be diſcharged by the kidneys, before one would have thought they had got into the blood.

and fifty-four ounces in *South Carolina;* which, though it has been commonly reckoned the total of the perſpiration, is really no more than its exceſs above the quantity of fluid taken in by the abſorbent veins of the ſkin, *fauces*, and lungs.

ALTHO' in vegetables, the veſſels which perſpire in the heat of the day, frequently aſſume a contrary office in the night-ſeaſon, and imbibe the dew and watery particles then floating in the air; yet it does not ſeem probable, that the exhaling or perſpiring veſſels of animals can thus become imbibing ones, or that the moiſture of the air can be, by them, conveyed into the blood: ſince any motion in theſe veſſels, from their extremities to their larger trunks, muſt be in oppoſition to the courſe of the arterial fluids.

THE imbibition by the veſſels of the ſkin is performed in the ſame manner as in the other abſorbents; only it is probable, that the perpetually varying oſcillations of the external air may concur in promoting it.

ALTHO' the exhalations from animal, vegetable, and mineral bodies, may be tranſmitted,

mitted, along with the watery particles in the air, into the blood, by the abſorbent veins of the ſkin and lungs, and thus account for peſtilential and epidemical diſeaſes raging at particular ſeaſons; yet it is by no means probable, that *elaſtic* air can be imbibed by theſe veſſels, and thus conveyed into the blood: for it has been obſerved, that air moves with great difficulty thro' capillary glaſs tubes, though ſome hundred times larger than the pores of the ſkin *: and it is well known, that water and other fluids can penetrate many ſubſtances thro' which air cannot paſs.

THIS obſervation of the difficulty with which air moves through capillary tubes, may ſerve to determine a controverſy which has long ſubſiſted amongſt phyſiologiſts, *viz.* Whether or not any *elaſtic* air enters into the blood by the lungs? For, ſince a few drops of water, with ſmall portions of air between them, in a capillary tube, require a greater force to make them aſcend, than

---

* Aërem vero non niſi tardè et cum quadam tenacitate per hos tubos moveri, ſemper docuit experientia; aëri enim ineſt ſpecies quædam tenacitatis aut immobilitatis. Muſchenbroeck De tub. capill. vitr. cap. 1. exp. xi.

than that with which the tube attracts the particles of the water *; it muſt follow, that, if any *elaſtic* air were admitted into the abſorbent veins of the lungs, it would not only not move through them itſelf, but would hinder their taking up, by their attraction, any other fluids.

The prodigious ſwelling of animals in an exhauſted receiver, further ſhews, that air cannot readily paſs through the ſmall pores of the ſkin and lungs. Nor is it any objection to this doctrine, that air has been found in the cavities of the heart; ſince, in a morbid ſtate, this might ariſe from the blood, of which air is a conſtituent part, as well as of other fluids †.

It is very obſervable, that air injected into the veins of an animal, produces obſtructions, concretions in the blood, and ſudden death; which effects, however, may be eaſily accounted for, from the power which air has of coagulating blood, and from the ſurpriſing influence it has in ſtopping the motion of water, even in large pipes,

---

* Muſchenbroeck, loc. citat.

† *Hales*'s Statical Eſſays, vol. 1. chap. vi.

pipes, eſpecially when lodging in their flexures *.

But to return; as the effluvia of different ſubſtances floating in the air, are, by means of the cutaneous abſorbents, conveyed into the blood, ſo likewiſe are the finer parts of plaiſters, cataplaſms, fomentations, and all other external applications: which ought therefore to be conſidered, not only as having a topical influence, but alſo as acting upon the whole body by their ſubtiler parts, which are mixed with the blood and other fluids.

It may be thought a difficulty, that quick-ſilver applied in the form of an ointment, ſhould be taken in ſo readily by the abſorbent veſſels of the ſkin; ſince, as has been obſerved above, it paſſes through the inteſtines without getting into the lacteals. But this happens from the particles of the mercury being extremely divided, and ſo united with thoſe of the greaſe as to enter the pores of the ſkin along with them: for, though quick-ſilver is repelled by capillary glaſs tubes, yet, if their internal ſurface is run

---

* Philoſoph. Tranſact. No. 393.

run over with melted greaſe, it will be attracted by them *.

We are told, that, upon opening the bodies of ſuch as had taken mercury in large quantities, this fluid has been, ſometimes, found in the cells of the bones and elſewhere †; the reaſon of which may be eaſily underſtood from what has been ſaid above: for, if the very ſubtile and greatly divided particles of mercury, after they are thrown out, by the exhaling arteries, along with the finer parts of the blood, into any cavity of the body, ſhould unite by their ſtrong mutual attraction, ſo as to form globules, whoſe diameters are larger than the diameters of the abſorbent veins, 'tis evident, they could never be taken up by theſe veſſels, but muſt remain for ever in ſuch cavity.

To conclude our obſervations on the abſorbent veſſels of animals; It may not be improper to take notice, that there are,

---

* Memoires de l'Academ. des ſciences, an. 1724.; and Muſchenbroeck De tub. capill. cap. iv. exp. 12. cor. 2. and cap. vii.

† Wepfer De apoplex. p. 277.; and Mead on poiſons, edit. 3.

upon the internal ſurfaces of the follicles and ſecretory and excretory ducts of the glands, bibulous veins, whoſe office is to carry off thoſe fluids which would be improper to enter into the ſeveral ſecretions. And, if we ſuppoſe theſe abſorbent veſſels, like other capillary tubes, to attract, according to their different natures, different fluids more or leſs ſtrongly, we ſhall ſee one great cauſe of the various ſecretions performed in the bodies of animals.

II.

OBSERVATIONS

ON THE

SENSIBILITY and IRRITABILITY

OF THE

Parts of MEN and other ANIMALS.

Occaſioned by *M. de Haller*'s late Treatiſe
on theſe Subjects.

Spiritus intus alit; totamque infuſa per artus
Mens agitat molem——— VIRG.

The THIRD EDITION.

ADVERTISEMENT

to the ſecond EDITION.

M. de HALLER *having, in the fourth volume of the* Memoires ſur les parties ſenſibles et irritables, *given a particular anſwer to the firſt edition of the following Obſervations, I have, in ſome of the notes to this edition, obviated ſuch of that learned author's objections as appeared to be of leſs moment, reſerving the principal points in debate between us, to be conſidered in the* Appendix.

Edinburgh, June 16. 1761.

OBSERVATIONS

ON THE

SENSIBILITY AND IRRITABILITY

OF THE

Parts of MEN and other ANIMALS.

PART I.

Of Senſibility.

THE truly learned and juſtly eſteemed *M. de Haller*, in his late treatiſe *De partibus corporis humani ſenſibilibus et irritabilibus* *, has favoured the world with an account of many new and curious experiments; from which he has frequently drawn ſuch concluſions as, if juſt, muſt neceſſarily produce conſiderable changes both in the theory and practice of the medical art. Being ſenſible how contrary his doctrine is, in many things, to the

---

* Acta Gottingenſ. vol. II. ad an. 1752. pag. 114, &c.

the received opinion of almoſt every phyſician, antient as well as modern, he has been at uncommon pains in making many and repeated experiments; as much to overpower the incredulous by their number, as to ſecure himſelf from any chance of being deceived *.

Opinions, *even* purely theoretical, ſhould not be let paſs, if there is any fallacy in them: but when propoſitions, founded on experiments, and ſupported by men of high character, are advanced, by which practitioners in medicine may be led into errors; it becomes the duty of every lover of the healing art, to prevent their being generally received as truths.

If the concluſions in the treatiſe above quoted ſhall be thought juſt, phyſicians and ſurgeons will certainly treat their patients in a manner very different from what they have hitherto done; whereby, if there be a miſtake in the doctrine, many lives may be endangered or loſt. It ſeems to be of ſome conſequence, therefore, to conſider this matter with attention, and to examine particularly, How far *M. de Haller*'s ſyſtem of ſenſibility is, or is not, well founded.

SECT.

---

* Acta Gottingenſ. vol. II. p. 115.

SECT. I.

OUR author, in treating of the ſenſibility of the ſeveral parts of the human body, reckons among the inſenſible parts, the tendons, *aponeuroſes*, ligaments, *capſulæ* of the articulations, *perioſteum*, bones, marrow, *dura* and *pia mater*, *pleura*, *peritonæum*, *pericardium*, *mediaſtinum*, and *cornea*.

1. HE tells us, that living animals, whoſe tendons were cut, burnt, pricked, or torn, ſhewed no ſigns of uneaſineſs; and, when a little part of the *tendo Achilles* was left intire, they walked without any ſeeming pain *.

2. WHEN the ligaments and *capſulæ* of the articulations were pricked with a needle, ſcraped with a knife, or had oil of vitriol or *butyrum antimonii* applied to them, the animals ſhewed no ſenſe of pain †. The wounds of theſe parts and of the tendons were followed with no bad ſymptoms, and were cured without any other remedy than

---

* Act. Gotting. vol. II. p. 120.

† Ibid. p. 122. et 123.

than the *ſaliva* of the animal, and ſometimes without this *.

3. THE *perioſteum*, when wounded, torn, or burnt, cauſed no pain to the animals †.

4. HE allows feeling to the teeth, but not to the other bones, becauſe they are not furniſhed with nerves, and becauſe he has ſeen the ſkull trepanned, without giving pain, in perſons who were poſſeſſed of all their ſenſes ‡.

5. HE denies feeling to the marrow, not from any experiments of his own on living animals; but becauſe it is a fatty ſubſtance and deſtitute of nerves ‖.

6. WHEN the *dura mater* was cut or lacerated, or burnt with oil of vitriol, ſpirit of nitre, and *butyrum antimonii*, the animal ſeemed to have no feeling of the injury §.

7. WHEN the *pia mater* was burnt by touching it with *butyrum antimonii*, the animals neither cried, nor were they convulſed; but, as ſoon as the brain itſelf was wounded, the body of the animal was twiſted

---

* Act. Gotting. vol. II. p. 121. et 223.

† Ibid. p. 123.

‡ Ibid. p. 124.

‖ Ibid. p. 125.

§ Ibid. p. 126.

twiſted and diſtorted with violent convulſions[*].

8. The *peritonæum*, *pleura*, and *pericardium*, when laid bare and cut, or otherwiſe irritated, produced no change in the animal[†].

9. He denies feeling to the *mediaſtinum*, not upon the authority of any experiments, but becauſe, like the *pleura*, it is a membrane and deſtitute of nerves[‡].

10. He reckons the *cornea* inſenſible, becauſe its nerves cannot be demonſtrated, and it is often pierced with a needle without giving pain[‖].

Besides the inſenſible parts above mentioned, there are others which, according to *M. de Haller*, have either no ſenſe of feeling, or a very obſcure one; and theſe are the arteries, veins, glands, and *viſcera*, *viz.* the lungs, liver, ſpleen and kidneys, which, when pricked, cut, or otherwiſe irritated, ſhewed nothing like feeling[§].

The concluſions which our author draws

---

[*] Act. Gotting. vol. II. p. 130.

[†] Ibid. p. 130.

[‡] Ibid. p. 131.

[‖] Ibid. p. 133.

[§] Ibid. p. 131. and 132.

draws from the above experiments, may be reduced to the three following.

1ſt, THAT the tendons, ligaments, *capſulæ* of the joints, *dura mater*, *pleura*, and other membranes, are quite inſenſible.

2dly, FROM the inſenſibility of theſe parts, and the difficulty of tracing, by diſſection, any nerves to them, he concludes that they have none, and that this is the reaſon why they are deſtitute of feeling.

3dly, HE thinks it follows, That thoſe parts which, from his experiments, he concludes to be inſenſible, have been unjuſtly accuſed by phyſicians as the ſeat of many painful diſeaſes. Particularly, That the pain, ſwelling, and inflammation which have often followed venæſection in the flexure of the arm, have not been owing to the tendons or *aponeuroſes*, in that part, being pricked by the lancet, but to the median nerve or ſome branch of the muſculo-cutaneous nerves being wounded*.---That we need be no way afraid of wounds of the tendons, whether they be cut, pricked, burnt, or otherwiſe hurt.------That the *cephalæa* and *phrenitis* have not their ſeat in

---

* Act. Gotting. vol. II. p. 121.

in the *dura mater* [*].------That the ſkin or ſubcutaneous nerves are the ſeat of the violent pain with which arthritic patients are affected, and not the ligaments or *capſulæ* of the joints [†]. And that the pain of the pleuriſy has been without reaſon ſuppoſed to be owing to an inflammation of the *pleura*, which is void of feeling [‡].

IN the few obſervations which I propoſe to make on this doctrine, I ſhall, *Firſt*, Conſider the parts, reckoned inſenſible by *M. de Haller*, in a ſound natural ſtate, ſuch as they were in his experiments; and, *2dly*, When they are affected with diſeaſes, whether in conſequence of ſuch experiments, or from other cauſes.

S E C T. II.

1. IN making or relating experiments, with a view to diſcover the ſenſibility or inſenſibility of the ſeveral parts of animals, particular regard ſhould be had to an obſervation made by *Hippocrates*, above two thouſand

---

[*] Act. Gotting. vol. II. p. 126.

[†] Ibid. vol. II. p. 122. and 123.

[‡] Ibid. p. 130.

thouſand years ago, *viz.* That a greater pain deſtroys, in a conſiderable degree, the feeling of a leſſer one *; an obſervation, the truth of which is confirmed by the daily experience of every phyſician. Thus, pricking any part of the body ſo as to give conſiderable pain, will ſo obliterate the irritation in the left orifice of the ſtomach, which is the cauſe of the hiccup, as inſtantly to put a ſtop to this convulſive motion. If a lighted candle be brought near a perſon whoſe eyes are a little inflamed, it will give him a good deal of uneaſineſs: but, if he be placed firſt in the ſunſhine, the candle will not add ſenſibly to his pain.

When a frog's hinder-feet are pricked or otherwiſe wounded immediately after cutting off its head, it makes ſcarce any motions at all with its legs, and ſhews almoſt no ſigns of feeling; but, if the toes are pricked or cut ten or fifteen minutes after decollation, the legs and thighs are not only violently moved, but ſometimes alſo the trunk of the body. Now, if in this caſe, as we ſee, the great pain occaſioned by cutting off the head rendered the animal

---

* Aphor. lib. 2. No. 46.

animal for ſome time inſenſible when its toes were wounded; is it to be wondered at, that, after the more ſenſible parts were cut, thoſe animals which *M. de Haller* opened ſhewed no ſigns of pain, when the leſs ſenſible parts were wounded?

WHEN the *thorax* of a living animal is laid open, it does not ſeem to receive any additional pain by pricking or cutting its heart; no new convulſions are produced, nor any change in the body, except perhaps a quicker repetition of the heart's motion: does it follow from this, that the heart is deſtitute of feeling? No, ſurely; but only that, after the great tortures ſuffered by laying open the *thorax*, the new pain produced by wounding the heart is too ſmall to make any remarkable impreſſion upon a dying and half inſenſible animal.

DOES it not appear, from what has been ſaid, that a want of due attention to the above-mentioned maxim of *Hippocrates*, which is ſupported by the ſtrongeſt experiments and obſervations, may have given occaſion to ſome miſtakes with regard to

the degree of ſenſibility in many of the parts of animals? Thus, it will not follow, that the tendons, ligaments, *capſulæ* of the joints, *perioſteum*, and *dura mater*, are altogether deſtitute of feeling, becauſe no convulſive motions or other ſigns of acute pain appeared in the animals when they were cut, pricked, or torn; for this might be owing either to their not being endowed with any painful feeling, or to the greater pain occaſioned by cutting the ſkin, ſubcutaneous nerves, *&c.* in order to get at thoſe parts, the ſenſibility of which our learned author propoſed to try. The concluſion therefore which ſhould be made from his experiments, is, not that the parts above-mentioned are wholly deſtitute of feeling, but that they are much leſs ſenſible than many others, or than has been commonly believed by phyſicians.

2. With regard to the marrow which *M. de Haller* reckons inſenſible; *Duverney*'s experiments made on men *, (which have alſo

---

* Dans les hôpitaux, où voyant panſer ceux qui avoient eu un bras ou une jambe coupée, je pouvois voir la moëlle à decouvert,—toutes les fois que je la faiſois toucher un peu rudement, le malade donnoit auſſitot des marques d'une nouvelle douleur. Memoires de l'Acad. des Sciences 1700. edit. 8vo, p. 255.

alſo ſucceeded with my ingenious friend and colleague Dr *Monro ſenior*), and particularly his experiment made on a living animal before the Royal Academy of Sciences at *Paris* *, are ſufficient proof that this part is

---

* “Vous vous ſouviendrez, Meſſieurs, que je fis ſcier devant vous, par le milieu, l'os de la cuiſſe d'un animal vivant ; et, ayant fait ôter les chairs et les membranes, pour laiſſer le bout de l'os entierement à nud, comme tous ces ebranlemens et ces diviſions cauſoient de douleur tres cruelle a l'animal, j'eus la precaution d'attendre que cette douleur fût paſſée, et, quelque tems après, plongeant un ſtilet dans la moëlle, vous vites que l'animal donna auſſi-tôt des marques d'une tres vive douleur, ce que fut reiteré pluſieurs fois avec la même precaution, et avec la même ſuccès.”

Memoires de l'Academie Royale des Sciences 1700, edit. 8vo, p. 256.

M. de Haller, in anſwer to the authority of *Duverney* which I had cited, ſays, that “a ſingle experiment is not ſufficient to prove the ſenſibility of the marrow, which is evidently cellular, and whoſe nerves have not yet been diſcovered (*a*).” But in confirmation of the truth of *Duverney*'s experiments, I mentioned that they had alſo ſucceeded with Dr *Monro ſenior*; and have now to add, that Dr *Al. Monro junior*, having

(*a*) *Memoires ſur les parties ſenſibles, vol. 4. p. 109.*

far from being deſtitute of feeling: and the reaſons given by *M. de Haller* for his placing it among the inſenſible parts, are not of any weight, when compared with thoſe experiments; for the feeling of the marrow is not owing to its oil, but to the membranes containing this oil; and the experiments which demonſtrate its ſenſibility prove that theſe membranes are furniſhed with nervous filaments, although they may be too ſubtile to be traced by the knife of the moſt accurate anatomiſt.

3. The *tunica cornea* is ſo far from being inſenſible, as Dr *Haller* would perſuade us, that any one may be ſoon convinced of the contrary by an eaſy experiment upon his own eye; for, when the *cornea* is touched with the point of one's finger, a very ſenſible

---

ving been preſent laſt ſummer (1760) at the amputation of the arm above the elbow, in a man of about forty, on account of a gun-ſhot wound in the hand, and a ſupervenient mortification, after the arm was taken off, he preſſed upon the bone, the marrow, and the muſcles repeatedly with the point of his finger; when the bone was touched the patient felt nothing, touching the muſcles occaſioned little pain; but he complained conſiderably as often as the Doctor preſſed the marrow with his finger.

ſible pain is felt: and it is well known, that powder of tobacco, or any acrid liquor applied to the *cornea*, excites a very painful ſenſation. Though the ſclerotic coat of the eye is far from being deſtitute of feeling, yet I have found it to be leſs ſenſible than the *cornea*, by touching both, not only with the point of my finger, but alſo with a bit of ſoft ſilk or linen *.

HAVING

---

* *M. de Haller* remarks, that in theſe experiments, I only touched the conjunctive (*a*), which is certainly true; nor was I ignorant of this. But as *M. de Haller* had pronounced the *cornea* in general to be inſenſible, and had made no exception in favour of the conjunctive which covers it, my experiments were certainly in point, and the concluſion from them juſt: and it will be found very difficult to prove by any experiment, that the pain occaſioned by cutting the *cornea* is not partly owing to this membrane, as well as to the conjunctive.

M. de Haller, unwilling to allow ſenſibility even to the conjunctive, aſcribes the pain occaſioned by touching the *cornea* to ſmall branches of the fifth pair of nerves which run between theſe membranes. But ſuppoſing the exiſtence of nervous branches between the *cornea* and conjunctive, as well as between this laſt and the ſclerotic, although no anatomiſt has yet demonſtrated the former; yet the pain occaſioned by touching the

cornea

(*a*) Memoires ſur les parties ſenſibles, vol. iv. p. 59. and 108.

Having had lately occaſion to be preſent at the extraction of the cryſtalline *lens* in Mr *Sharp*'s way *, I inquired particularly of the patient, Whether he felt any pain when the *cornea* was firſt pierced with the knife employed in that operation? he told me, He thought the pain was much the ſame with what he uſed to feel when the ſkin of his arm was cut in blood-letting. It deſerves however to be remarked, that, though the ſkin and *cornea* are both endowed with a very conſiderable degree of ſenſibility; yet, when they are cut quickly with a very ſharp inſtrument, there is

---

cornea very gently, or the ſenſation produced by the cool air blowing on it, cannot well be conceived to be owing to any thing elſe than the ſenſibility of its exterior covering. Nay, if the *cornea* itſelf were not more ſenſible than the ſclerotic, why ſhould the conjunctive feel more acutely where it covers the former, than where it is contiguous to the latter? The conjunctive, where it covers the *cornea*, is certainly one of the moſt ſenſible parts of the whole body, and leaſt able to bear any hurt, or the application of any acrid ſubſtance. Nor could its ſenſibility be ſo great, if it were owing ſolely to ſome branches of nerves running between it and the *cornea*.

[*] Philoſoph. tranſact. vol. xlviii. p. 1. p. 322.

is much leſs pain felt than one would imagine. Thus, when the ſkin is ſlightly wounded in ſhaving one's beard with a razor, the blood that follows is often the firſt thing that lets one know of any ſuch thing having happened: and this, together with the pain occaſioned by holding the eye firm in its orbit, and the concern the patients are generally under, may very well account for their being ſometimes ſcarce ſenſible of any pain when the *cornea* is pierced with a ſharp needle. But upon the whole, it appears, that the *cornea* is poſſeſſed of a remarkable degree of ſenſibility; and conſequently, that *M. de Haller*'s poſition, That all membranes are deſtitute of feeling *, muſt admit, at *leaſt*, of one exception †.

4. OUR

---

* Act. Gotting. vol. II. p. 130.

† *M. de Haller* repreſents me as inconſiſtent with myſelf in giving the *cornea* as an inſtance of a membrane that is ſenſible, after having owned that, when pierced with a ſharp needle, it often occaſions little pain (*a*); but there is really no inconſiſtency here, ſince I have obſerved, at the ſame time, that the ſkin, which is among the moſt ſenſible parts, feels generally leſs from being ſlightly cut with a ſharp razor, than the *cornea* does when pierced with a needle.

(*a*) Vid. *Memoires ſur les parties ſenſibles*, &c. vol. iv. p. 108.

4. Our author allows the kidneys either no feeling, or a very obſcure degree of it; becauſe he could obſerve no ſigns of pain in the animals whoſe kidneys he cut or pricked with a knife: but, after cutting the ſkin, abdominal muſcles, *&c.* and diſplacing the inteſtines in order to get at the kidneys, it was ſcarcely to be expected, that the animals would ſhew any tokens of additional pain when theſe organs were wounded, unleſs they had been equally, or more ſenſible than the parts before diſſected.

A Physician of my acquaintance, who had occaſion to ſee the operation of nephrotomy performed a few years ſince, was told by the patient that, when the kidney was opened, he felt pain, though duller and leſs acute than when the ſkin was cut.

One inſtance of this kind is more deciſive in favour of the ſenſibility of the kidneys, than twenty experiments on brutes, who cannot inform us whether they feel a ſlight pain or none at all; and, if the kidneys be leſs ſenſible than the ſkin, we cannot

not expect that wounding them will add conſiderably to the pain which the animals ſuffered before from cutting the ſkin and muſcles.

It is very obſervable, that, while *M. de Haller* denies feeling to the kidneys, he allows it to the ureters: not becauſe animals, when theſe are cut or wounded, ſhew ſigns of greater pain than when the kidneys are treated in the ſame manner; but becauſe he ſuppoſes the ureters to be of the nature of the ſkin, and propagated from it *. And indeed, even the ſtrongeſt experiments upon brute animals would not have been ſufficient to have proved the ureters inſenſible in men; when ſtones paſſing from the kidneys to the bladder generally occaſion ſuch exquiſite pain. But, does not the acute pain always attending a *nephritis*, and ſometimes occaſioned by a ſtone lodged in the kidneys, ſhew, beyond doubt, that they are endowed with feeling as well as the ureters? while nothing can be concluded from *calculi* lying long in the kidneys without giving pain †, except that they were ſo ſituated as not to hurt them.

5. ALTHO'

---

* Act. Gotting. vol. II. p. 131.

† Ibid. p. 132.

5. ALTHO' brute animals ſhew ſmall ſigns of ſenſibility, when the glands are pricked, or have acrid things applied to them, immediately after the very ſenſible ſkin has been cut; yet, we know that a bruiſe on the teſticles often cauſes, inſtantly, ſuch exquiſite torture as to make men faint; and a blow on a woman's breaſt often excites, immediately, ſhooting pains in the gland there, though no mark of the bruiſe appears in the ſkin. Theſe are ſuch undoubted proofs of the ſenſibility of the glands, as no experiments made on brute animals will ever be able to overthrow.

6. M. DE HALLER allows the membranes of the *aorta* near the heart, and of the temporal, lingual, labial, thyroid, and pharyngean arteries, to be ſenſible; but thinks the coats of the arteries in other parts of the body have either no feeling, or a very obſcure degree of it; though it does not appear from his experiments, that animals complained more when the former than when the latter were irritated. In this caſe, he relinquiſhes the appeal to experiment, and founds his opinion on his tracing nerves to the former, which

he

he could not do to the latter: an argument he makes uſe of upon ſeveral other occaſions, and which is next to be examined.

7. As our author not only founds his opinion of the inſenſibility of many parts of the body upon experiments made on living animals, but alſo on their being deſtitute of nerves; we ſhall briefly conſider, whether, from the real or ſeeming inſenſibility of any part, or from anatomiſts being unable to demonſtrate its nerves, we are intitled to conclude that it has none.

ALTHO' the tendons are quite inſenſible according to *M. de Haller*, and their nerves can ſcarcely be demonſtrated by anatomiſts; yet, we are convinced, that the tendons are not deſtitute of nerves, from the following obvious obſervation. In fœtuſes and new-born children, many parts which afterwards, in an adult ſtate, become tendinous, are muſcular, or partly ſo; and as animals advance in age, the proportion of the tendinous to the muſcular part gradually increaſes; we muſt either, therefore, deny

nerves to the muſcles, or allow them to the tendons alſo *.

ALTHO' we cannot trace nervous filaments to the ſmall arteries, we have reaſon to believe they are furniſhed with them; elſe, how could the diſtraction of their coats in inflammations occaſion ſuch acute pain †? I think we may conclude every part that is liable to be inflamed by irritation to be, in ſome degree, ſenſible and endowed

---

* The reader will obſerve, that from thoſe parts which were once muſcular becoming afterwards tendinous by age, I only conclude that they are not deſtitute of nerves, but not *that they are ſenſible*; which, however, *M. de Haller* has repreſented me as ſaying (*a*), although it be expreſsly contrary to my acknowledgement of the very obtuſe feeling of the tendons, and to what I have ſaid below, of the ſenſibility of the parts depending not *ſolely* on their having nerves, but on the diſpoſition and ſtate of theſe nerves.

† *M. de Haller* ſeems ſometimes to allow ſenſibility to the arteries, and at other times denies that they have any, becauſe, when they are tied, the animals do not ſeem to feel any pain. But it is to be obſerved, that the ureters, whoſe ſenſibility he acknowledges, may be tied or wounded with as little ſigns of pain as the arteries. Vid. Memoires ſur les parties ſenſibles, vol. iv. p. 87. et 110.; and Act. Gotting. vol. ii. p. 142.

(*a*) *Memoires ſur les parties ſenſibles, vol.* iv. *p.* 103.

ed with nerves; for, ſince the inflammation cannot in this caſe be owing to any increaſed force of the heart, the diſtenſion of the ſmall arteries, and the greater *impetus* of the blood in them, muſt be owing to an increaſed oſcillatory motion in the veſſels themſelves, excited by the unuſual irritation: but theſe motions of the ſmall veſſels being of a like kind with thoſe alternate contractions which are obſerved in muſcles whoſe fibres have been irritated, it will follow that thoſe veſſels partake of a muſcular nature, and conſequently have nerves like the other muſcles.

WITH regard to the membranes; ſince the *dura mater* and *pleura* are furniſhed with nervous filaments, which anatomiſts have been able to demonſtrate *, we may reaſonably conclude that the other membranes are not deſtitute of them; altho' they may be too ſmall to come under the eye of the beſt diſſector: this is certainly true of the *cornea* and membranes containing the marrow, which we have ſhewn, from undoubt-

---

* Winſlow Expoſition. anatom. ſect. ix. No. 55. and ſect. x. No. 47.

ed experiments, to be ſenſible, and conſequently not without nerves. It appears, therefore, that we can by no means conclude any part to be inſenſible, *merely* becauſe its nerves cannot be demonſtrated.

On the other hand, it is allowed, that we cannot certainly conclude, from a part's being furniſhed with nerves, that it is ſenſible at all, or in what degree: for the nerves muſt be in a certain ſtate to perform their offices rightly; and, in proportion as they recede from this, their ſenſibility will be more or leſs blunted. Examples will illuſtrate this.

The bones, which in a natural ſound ſtate are inſenſible, are neverthelefs moſt certainly furniſhed with nerves; as appears from the remarkable ſenſibility of the granulated ſubſtance which riſes from them after fractures, or their being chizelled, or when they exfoliate: this ſoft fleſh, however, gradually loſes its feeling as it grows harder, till being at laſt turned into a callous or oſſeous ſubſtance, it becomes wholly inſenſible.

The membranes of the *tela cellularis* are, in a natural ſtate, ſoft, flexible, and diſtenſile,

ſile, and have little feeling; but, in every wound or ulcer, when they acquire ſome more firmneſs, they are ſenſible of every touch and every acrid application, as ſurgeons ſee daily. After a cicatrice has ſome time covered the parts where the ſore was, and they have returned to their natural ſoftneſs, theſe cellular membranes loſe again their ſenſibility, as appears on making a new wound thro' the cicatrice; and recover it again, whenever they become firm and tenſe, by the new inflammation and ſuppuration.

THE *dura mater*, which, in a ſound ſtate, has but little feeling, granulates after the trepan, and feels every irritating ſubſtance applied to it; and the ſame thing happens to cartilages, ligaments, tendons, membranes, *&c.*

WITHOUT attention to this change in the firmneſs of parts, and its effect upon their nerves, we could never account for what has been obſerved above, *viz.* that the parts of muſcles, which in fœtuſes and children are lax contracting fibres and very ſenſible, become in a great meaſure inſenſible, in a ſound ſtate, when, by the

creature's advancing in age, they are compacted into tendons, as happens to many of them.

If ſenſibility, then, be a ſure mark of the exiſtence of nerves in any part of the body, there is not one that is deſtitute of them, altho' anatomiſts will never be able to demonſtrate them in every part.

From what has been ſaid, it may appear, that *M. de Haller*'s experiments on living animals do not ſufficiently prove the doctrine he would deduce from them; and that his argument for the inſenſibility of parts, taken from their nerves not being demonſtrable, is altogether inconcluſive. Let us next try what further light diſeaſes will throw upon this ſubject.

SECT. III.

If the parts reckoned inſenſible by *M. de Haller* were really deſtitute of nerves, it would follow, that they could in no caſe become the ſeat of painful ſenſation; and even ſuppoſing them furniſhed with nerves,

but poſſeſſed only of an obſcure degree of feeling, it may be thought, at leaſt, not probable that they can be the ſeat of thoſe painful diſeaſes commonly aſcribed to them. In order to ſet this matter in a proper light, it will be ſufficient to diſtinguiſh between parts in a ſound and in a diſeaſed ſtate. In a ſound ſtate, the feeling of many parts of the body is but very dull, which is altogether neceſſary to prevent the uneaſineſs we ſhould otherwiſe perpetually ſuffer, when our organs are ſtretched, preſſed upon, *&c.* in the common offices of life: ſuch parts, therefore, when cut or wounded, in a ſound ſtate, give little uneaſineſs; but, if afterwards an inflammation comes on them, they become extremely ſenſible, and their over-ſtretched veſſels and nervous filaments occaſion intenſe pain, by which we are excited to endeavour the cure of the diſeaſe.

It is certain, that thoſe parts which are moſt ſenſible in a ſound ſtate, acquire a more acute feeling when inflamed. Thus the ſtomach, which, in health, can bear the touch of wine, brandy, and other pungent

gent liquors, without being hurt, is, when inflamed, often brought into convulſions by the mildeſt drinks; and light, which gives no ſenſible pain to the eye in a ſound ſtate, becomes intolerable when this organ is inflamed. Nor can we doubt that the more inſenſible parts may acquire, when inflamed or otherwiſe diſeaſed, a remarkable degree of ſenſibility. Examples above recited have ſhewn this to be true of the bones, *tela cellularis*, and *dura mater*; and the following facts will ſhew the ſame thing, in other parts, reckoned either wholly, or almoſt wholly, inſenſible by our author.

As often as there is an inflammation, eſpecially when tending to ſuppuration, in any of the glands, as the parotids, tonſils, maxillaries, *mammæ*, *teſtes*, kidneys, *&c.* the patient is tortured with pain, often before the teguments are affected or even conſiderably ſtretched. And is not this a much better proof of the ſenſibility of theſe parts, than *ſchirri* and other indolent ſwellings are of the contrary?

The fore-part of the eye, when inflamed,

med, can bear the touch of no hard or acrid ſubſtance; and *fungi* riſing from it give very ſharp pain, when fretted.

In the rheumatiſm, joints, where the ſkin is unſtretched and of the natural colour, and where no muſcular fibres are placed, are ſeverely pained on the leaſt motion, tho' done without the effort of the patient; which muſt therefore depend on the ſenſible ligaments and tendons; ſince large branches of nerves, thus affected, would produce convulſions of the muſcles they ſerve, which do not happen: beſides, in theſe caſes, the pain is not felt where the large nerves are.

A contuſion, by a fall on the great *trochanter* of the thigh, without cauſing *ecchymoſis*, or ſwelling of the teguments, often brings, in a little time, racking pain on all the outſide of the thigh, leg, and foot; which continues obſtinately for months or years thro' the whole extent of the *faſcia lata.*

An inflammation of the *perioſteum*, as in the *panaris*, where the ſuppuration happens between this membrane and the bone, nay even

even the repletion of the veſſels of an overſtretched *perioſteum,* as by heat or food in venereal nodes, gives very ſharp pain. And, in the *ſpina ventoſa* and other ſuppurations of the marrow, pain is felt before any ſigns of the diſeaſe appear externally.

These obſervations ſeem to demonſtrate, beyond doubt, that many of thoſe parts which *M. de Haller* would have us believe to be inſenſible, are often the ſeat of remarkable pain in the human body; and, I cannot help thinking, that, in other examples, where he endeavours to aſſign a different ſeat of the painful ſenſation, he is miſtaken, and is laying the foundation of dangerous practice. It will, therefore, be worth while to examine theſe caſes.

1. He imagines that the pain, ſwelling, and inflammation of the arm, which have ſometimes followed the opening of the *median* vein, muſt have proceeded, not from a wound of the tendon of the *biceps* muſcle, but of the *median* or ſome other nerve. But, if this were the caſe, why ſhould not ſimilar ſymptoms ſometimes follow blooding in the cephalic or jugular veins? In opening

pening the jugular vein, ſome nervous filaments are frequently wounded, and often occaſion a ſharp pain, as if the point of the lancet had been left in the wound; this, however, goes off in a day or two, or ſooner, without leaving any bad conſequence. But the miſchiefs which have followed blooding in the *median* vein are of a different kind. Tho' little or no pain is felt at firſt, yet afterwards, not only the whole arm is violently pained and ſwelled, but a particular hard ſwelling is often formed in the place where the wound was made, from which a thin lymph iſſues; and the patient does not recover the full uſe of his arm for ſeveral months; nay, ſometimes loſes the motion of the elbow-joint altogether. And that a wound in the tendon is, at leaſt, ſometimes the cauſe of thoſe ſymptoms that follow blood-letting in the flexure of the arm, appeared evidently in a patient who died in this place, ſome years ago, of a fever occaſioned by the pain, ſwelling, and inflammation conſequent upon opening the median vein of the right arm, the *tendo bicipitis* of which was ſwelled to near ten times its natural bulk.

How very ſenſible tendons may become, when inflamed, appears from various obſervations; particularly one mentioned by the Baron *Van Swieten*, of a Nobleman who was ſeized with moſt terrible convulſions over his whole body the moment his ſurgeon took hold of one of the tendons near his ancle, miſtaking it for a part of the fatty membrane *.

2. Our author aſcribes the pain of the gout to the ſkin or ſubcutaneous nerves, and not to the *capſulæ* or ligaments of the joints affected. But does not the rigidity of the joints, which the gout at laſt produces, ſhew, that its ſeat is deeper than the ſkin or nerves below it; and that the ligaments of the articulations, and tendons of the muſcles which ſerve for their motions, are affected?

When one ſprains his wriſt or ancle, there is often no great pain felt immediately; but ſoon after, when the over-ſtretched parts begin to ſwell and inflame, a conſiderable pain enſues, which is greatly increaſed if the joint be moved. Does not the pain in this caſe proceed chiefly from the over-

* Comment. in Aphor. Boerhaave, vol. I. p. 241.

over-ſtretched ligaments or tendons? It will be hard to perſuade phyſicians, that it is owing to any hurt received by the ſkin or ſubcutaneous nerves. And, if the ligaments or tendons may be affected with pain from being too much ſtretched, why may not they be the principal ſeat of that pain which affects the joints of gouty patients?

CHALK-STONES in a joint frequently give ſharp pain before they pierce the capſular ligament, and before the ſkin is much ſtretched or red. Further, without allowing ſenſibility to the ligaments, let any one try to explain what my learned colleague Dr *Monro ſenior*, and, I dare ſay, many others have oftener than once ſeen in practice. A pea-iſſue, for a dropſy of the knee, put in with a cauſtic or a knife, and dreſſed with the pea a conſiderable time, created little uneaſineſs to the patient; but, after a puncture of a lancet made, very near to where the iſſue was, through the *capſula* of the joint, to let out the water, moſt racking pain and inflammation enſued, which

brought the patient to the brink of the grave *.

3. OUR author is of opinion, that the inſenſible *dura mater* cannot be the ſeat of a headach or *phrenitis*. But how little ſenſible ſoever this membrane may be in a natural ſtate, yet, if it may be affected with pain as often as it is inflamed or obſtructed, it may ſtill be, in many caſes, the ſeat of thoſe diſeaſes. In patients who have died of a *phrenitis*, the *dura* and *pia mater*, as

---

* In anſwer to this, *M. de Haller* has thought proper to remark, that Mr *Warner*, who is much better acquainted with ſurgery than Dr *Whytt*, recommends the opening the capſular ligament as the only effectual cure in a dropſy of the articulations (*a*). The impropriety of this remark muſt appear in a ſtrong light to the reader, when he obſerves that what I aſſert here is *ſolely* upon the authority of Doctor *Monro ſenior*, whoſe ſkill in ſurgery, as well as his accuracy in making obſervations, is too well known to be called in queſtion.

But further, the pains which followed the opening of the capſular ligament in the only caſe which is mentioned by Mr *Warner*, though they were leſs violent than is uſual in ſuch caſes (*b*), ſhew that this ligament, however little feeling it has in a ſound ſtate, may, when diſeaſed, become the ſeat of painful ſenſation.

(*a*) *Memoires ſur les parties ſenſibles*, vol. iv. p. 59.

(*b*) Vid. Philoſ. Tranſact, vol. xlix. p. 457.

as well as the cortical ſubſtance of the brain, have been found inflamed, ſuppurated, and mortified: and in thoſe who, after recovering once and again of a *phrenitis*, have died of other diſeaſes, the *dura* and *pia mater* have been found much thicker and harder than uſual *.

As the headach generally attending fevers often begins ſeveral days before any ſigns of a *delirium* appear, we cannot aſcribe it to an obſtruction in the cortical part of the brain, but in the *dura* or *pia mater*. Nor can this headach have its ſeat in the exterior teguments of the ſkull; otherwiſe the pain would be increaſed by preſſing the part chiefly affected, as often happens in thoſe periodical headachs which ſeem to have their ſeat in the ſubcutaneous nerves, or *pericranium*.

Lastly, *M. de Haller* thinks, that the intercoſtal muſcles, or large nerves running between the ribs, are the ſeat of the pain of the pleuriſy, and not the *pleura* itſelf, which is inſenſible. But, if this membrane, notwithſtanding its ſmall degree of ſenſibility

---

* Van Swieten Comment. in Boer. Aphor. vol 2. p. 694.

ſenſibility in a ſound ſtate, may be affected with great pain when inflamed; it will hardly be doubted that it is ſometimes the ſeat of the pleuriſy: ſince, in patients who have died of this diſeaſe, the *pleura* has been found inflamed and ſuppurated [*].

But, beſides the inſenſibility of the *pleura*, *M. de Haller* has brought another very plauſible argument to prove, that the pleuriſy can never have its ſeat in this membrane, *viz.* the patient's feeling the greateſt pain in inſpiration when the ribs are brought nearer each other, and conſequently when the *pleura* is leſs upon the ſtretch than it was in time of exſpiration. But this learned author has long ago very juſtly obſerved, that ordinary and gentle inſpiration in men, is chiefly performed by the diaphragm, while the intercoſtal muſcles are ſcarce employed at all [†]: wherefore in inſpiration, which pleuritic patients perform with great caution, the ribs may be ſuppoſed to alter their ſituation very little [‡]; but,

---

[*] Van Swieten Comment. vol. 3. p. 8.

[†] Praelect. in Inſtitut. med. Boerhaav. vol. iv. No. 615. not. (*a*).

[‡] It is ſomewhat ſurpriſing, that *M. de Haller* ſhould have mentioned the approach of the ribs in inſpiration,

but, as the inferior part of the *pleura* muſt be ſomewhat ſtretched by the deſcent of the diaphragm in inſpiration, it is no wonder the pain ſhould be then moſt acute.

In women, eſpecially ſuch as are pregnant, who uſe the intercoſtal muſcles more in ordinary inſpiration than men, the *pleura* will be more ſtretched at that time than during exſpiration; becauſe the cavity of the *thorax* is increaſed in wideneſs and depth, as well as length.

With regard to what *M. de Haller* ſays of the ribs approaching each other in inſpiration; though this is certainly true of the ſuperior ribs, yet I have ſome doubt, whether it be ſo in the inferior ones: for, in a very full inſpiration, I can with my fingers plainly feel the ſix or ſeven inferior ribs recede from each other, and approach again in the ſucceeding exſpiration

---

ſpiration, as an argument to prove, that the pleuriſy never has its ſeat in the *pleura*; after having formerly told us, that pleuritic patients don't uſe the intercoſtal muſcles at all, but breathe by means of the diaphragm alone. Praelect. in Inſtitut. med. Boerhaav. vol. iv. No. 615. not. (*a*), and No. 619. not. (*c*).

tion *. Wherefore it muſt appear, that the increaſe of the pleuritic pain in time of inſpiration can be no proof, that the diſeaſe has not its ſeat ſometimes in the *pleura*.

UPON the whole, although *M. de Haller*'s experiments clearly ſhew, that ſeveral parts of animals are poſſeſſed of a much more obſcure degree of feeling than has been commonly imagined; yet it is hoped, the reader will, after weighing what has been ſaid, be far from pronouncing them altogether inſenſible, or condemning the uniform opinion of phyſicians in all ages concerning the parts which are affected in many diſeaſes; and, inſtead of it, embracing a doctrine which is far from being ſufficiently proved, and may, if made a foundation for practice, be of fatal conſequence.

---

* The reaſon why not only the falſe ribs, but alſo ſome of the true ones, rather recede from than approach each other in inſpiration, may be underſtood from what is briefly ſaid concerning the motions of the *thorax*, by Dr *Monro*, in his Anatomy of the bones, edit. 5. p. 242.

PART II.

Of Irritability.

SECT. I.

ALTHO' many of the parts compoſing the human body are endowed with a conſiderable degree of elaſticity, whereby they reſtore themſelves when over-ſtretched; yet muſcular fibres alone are poſſeſſed of a proper contractile power, which is exerted, in conſequence either of an effort of the will, or of ſome *ſtimulus* applied to them or their nerves: by the former, voluntary motion is produced; by the latter, involuntary *. The learned *M. de Haller*, who chuſes to call the contractile power of irritated muſcles by the name of *Irritability*, has, by a variety of curious experiments upon living animals, ſhewn, that it is a property of all muſcular fibres; and that no part, which is not muſcular, is irritable, although, of the muſcular parts, ſome are more and others leſs ſenſible of irritation. But when, in his enumeration of

---

* Vid. an Eſſay on the vital motions, &c. ſect. i. and x.

of the parts of the body that are or are not irritable, he allows irritability to the lacteal veins, mucous glands, and ſinuſes, and yet denies it wholly to the kidneys and ureters, and almoſt wholly to the arteries, veins, and excretory ducts of the glands, we cannot help differing greatly from him: ſince theſe laſt parts are, at leaſt, as much muſcular as the former; and ſince our learned author's experiments on living and dying animals ſhew neither the one nor the other to be irritable *.

THAT the ſmall arteries are not deſtitute of irritability, may be demonſtrated by undoubted experiments. Thus, when an acrid cataplaſm is applied to the ſkin, or ſpirit of wine to the eye, whence proceeds the inflammation which is ſoon produced in the ſkin, and almoſt inſtantly in the eye? Not, ſurely, from any increaſed force of the heart or larger arteries, but from the irritated veſſels themſelves, which are agitated with ſtrong alternate vibratory contractions; by means of which the moment of the blood in them is greatly increaſed, and red globules are puſhed into thoſe veſſels

---

* Act. Gottingenſ. vol. ii, p. 139.——143.

veſſels which, in a ſound ſtate, only receive *ſerum* or lymph *.

---

* *M. de Haller* ſays, that the irritation of the ſmall arteries, if they are hollow muſcles, ought rather to empty them than increaſe their diameter (*a*). But a little attention will ſhow this opinion to be ill-founded. If an irritated artery could empty itſelf as eaſily as the bladder of urine, and were as ſlowly ſupplied with new fluids, an irritation of it would have the effect mentioned by my learned adverſary. But ſince the arteries are furniſhed with a continued ſupply of blood from the heart, it is eaſy to ſee, that as often as by any conſiderable *ſtimulus* the alternate contractions of the ſmall arteries of any part are greatly increaſed, the force of the blood muſt not only be augmented, but theſe arteries, as well as the ſmaller lateral branches which they ſend off, muſt be enlarged in their diameters, and contain a greater quantity of fluids than uſual, *i. e.* the part will be inflamed.

The increaſed heat, redneſs, and pulſation in an inflamed part can only be conceived to happen, either from an increaſed force of the heart and larger arteries, or of the ſmall veſſels themſelves. But we know that in many topical inflammations from external cauſes, the force of the heart and large arteries is not altered; wherefore the inflammation muſt in ſuch caſes be aſcribed to the increaſed alternate motion of the ſmall veſſels themſelves; for their continued ſpaſmodic contraction would occaſion a ſenſe of cold and a paleneſs, not a heat and redneſs.

(*a*) *Memoires ſur les parties ſenſibles*, &c. vol. iv. p. 113. & 114.

NOR can we conclude that the arteries are deſtitute of irritability, becauſe the *aorta* was not obſerved to contract itſelf when pricked with a ſharp inſtrument, or touched with acrid liquors *; ſince the ſame is true of the mucous glands and ſinuſes, which yet our author allows to be irritable †. And it is not improbable, that the ſmall capillary arteries may be more irritable than the *aorta* or larger ones; becauſe their muſcular coat, as it is called, is much leſs firm and tendinous.

FARTHER, *M. de Haller* reckons the lacteal veins irritable, becauſe, after death, they contract themſelves ſo as to expel the chyle and become inviſible ‡; but do not all the arteries of the body, ſmall as well as great, alſo contract themſelves after death, and puſh moſt of their blood forward into the veins?

Laſtly, Since an irritation of the ſalivary and lachrymal veſſels, and of the mucous ducts and ſinuſes, increaſes the motion of the fluids in them, it will be difficult to give a reaſon why it ſhould not have a ſimilar effect in the ſmall arteries every where through the body.

---

* Act. Gottingenſ. vol. ii. p. 141.

† Id. p. 143.

‡ Ibid. p. 142.

veins? And is not this coarctation of the lacteals owing more to the elaſticity of their coats now increaſed by cold, than to a proper muſcular contraction? However, if the lacteals be irritable, as is, I think, very probable, though for other reaſons than the one now mentioned; it will follow, that the lymphatic and other veſſels of the body are ſo likewiſe: for the lacteals are only a kind of lymphatic veins ariſing from the villous coat of the guts, which, on account of the colour of their fluid, have got the name of *lacteal.* Nor have we any reaſon, from their muſcular ſtructure, to aſcribe irritability to the lacteals and thoracic duct, more than to the other veſſels of the body.

WITH regard to the veins, I ſhall only obſerve, that, ſince the alternate contractions of the trunks of the *venæ cavæ* near the heart, ſhew them to be poſſeſſed of a remarkable degree of irritability; it is not probable that the other veins are *wholly* deſtitute of it. I know that *M. de Haller* denies any proper motion to the *cava*, and aſcribes its ſeeming alternate dilatation to the

the blood puſhed back into it by the contracting auricle *. But, if this were true, how could the *cava* contract five or ſix times before the right auricle performed ſo much as one pulſation, as *Steno* has obſerved in rabbits †? or how could it poſſibly continue its alternate motions, not only for a conſiderable time after the right auricle had ceaſed to move ‡, but even after the heart, together with this auricle, was intirely ſeparated from it ‖? Theſe facts ſhew ſo clearly that the motions of the *venæ cavæ* do not proceed from the alternate contractions of the right auricle, as to make any further obſervations on our learned author's miſtake, as to this matter, altogether needleſs §.

DOES

---

* Primæ lineæ phyſiolog. edit. 2. No. cxiii.

† Bartholin. Epiſt. med. cent. iv. p. 3.

‡ Bartholin. Epiſt. cent. iv. p. 110.; and Eſſay on vital and involuntary motions, p. 354.

‖ Walæus de motu ſang. ad. fin.; Anatom. Bartholin. p. 783.

§ *M. de Haller* in his later writings, acknowledges the motions of the *vena cava* to be owing to its own fibres; and has further ſhewn that the pulmonary veins near the left ventricle of the heart are endowed with the

Does not the ſudden flow of pale urine in hyſteric caſes, and the increaſed derivation of *ſaliva* into the mouth of a hungry perſon from the taſte or even the ſight of grateful food, ſhew that the ſecretory veſſels of the kidneys and excretory ducts of the ſalivary glands are, in ſuch caſes, agitated with an unuſual oſcillatory motion, and conſequently not deſtitute of irritability? Nor ought *M. de Haller* to have denied this power to the veſſels of the kidneys and excretory ducts of the glands: ſince he allows it to the lachrymal glands and mucous ſinuſes, becauſe they pour forth their fluids more copiouſly when ſtimulated; although his experiments diſcovered no ſigns of irritability in them *.

When a ſtone paſſes from the kidneys to the bladder, does not the irritation of the ſharp ſtone occaſion ſome kind of ſpaſmo-

---

the ſame power: But from his remarks on this paſſage, the reader would imagine that *M. de Haller* had never aſcribed the motion of the *vena cava* to its dilatation by the blood puſhed back into it by the right auricle, and that I had charged him falſely with this opinion (*a*); which neverthelеſs is to be found in No. cxiii. of the ſecond edition of his *Primæ lineæ phyſiolog.*

* Act. Gotting. vol. ii. p. 143.

(*a*) *Memoires ſur les parties ſenſibles*, vol. iv. p. 116. 117.

dic contraction in the *ureter:* and does not a large doſe of *opium* facilitate its paſſage, by abating or deſtroying the painful feeling, and conſequently leſſening the conſtriction of the *ureter?* This canal, therefore, ſeems to be poſſeſſed of ſome kind of irritability, notwithſtanding *M. de Haller* tells us it was, in the animals he opened, inſenſible of the *ſtimulus* of oil of vitriol *†.

If our author's experiments diſcovered no kind of irritability in the blood-veſſels, lacteals, glands, and mucous ſinuſes, it will not follow that the *iris* is deſtitute of this power, although it did not appear to contract when irritated with a knife ‡.

The Doctor adds, that the dilatation of the pupil cannot be owing to any muſcular power, becauſe it becomes wideſt at death or immediately after it ‖. I have elſewhere obſerved that the dilatation of the pupil is owing

---

* Act. Gotting. vol. ii. p. 142.

† As a further proof of this, my ingenious Colleague Dr *Alexander Monro junior* informs me, that in a pig which was ſtrangulated and half dead, he obſerved the *ureter* to contract very remarkably, when he touched it with the point of his finger; nay, when he moved the point of his finger along the ſurface of the ureter, a ſucceſſive contraction of this canal was produced from above downwards.

‡ Act. Gotting. vol. ii. p. 143. ‖ Ibid.

owing to the longitudinal fibres of the *uvea*, which, by their natural contractility, retract its edges, when the orbicular muſcle is not excited into contraction by the action of light on the *retina* *: at death, therefore, when the eye becomes inſenſible, the pupil muſt be very wide; but, ſome time after death, as the accurate *Winſlow* has always obſerved †, and I have alſo ſeen the pupil become narrower, becauſe the longitudinal fibres of the *uvea* loſe their tone, become flabby, and are elongated ‡. Nor

---

[*] Eſſay on vital motions, ſect. vii.

[†] Memoires acad. des ſciences 1721. edit. 8vo. p. 416.

[‡] *M. de Haller* mentions in ſeveral of his experiments, that he obſerved the pupil very wide in animals not only at the time of death, but for ſome hours after it: Nor is this inconſiſtent with *Winſlow*'s having almoſt conſtantly found the pupil of a moderate ſize in the human body a day or two or perhaps longer after death; for the pupil, which continues wide for ſeveral hours after death in men, as well as in other animals, becomes narrower as ſoon as the fibres of the *uvea* have loſt their tenſion and elaſticity: Nay, I have obſerved the pupil, after having been remarkably wide and deſtitute of all motion for ſome time, become narrow even a day or two before death in two boys who died of a dropſy in the ventricles of the brain. In theſe patients, ſp. of ſal. ammon, held to the noſe, or a ſpoonful of

does *M. de Haller* ſeem to have attended to what is ſaid in page 111. and 129. of my Eſſay on the vital motions, *&c.* when he mentions the dilatation of the pupil at death, as a clear proof that it is not owing to the contractile power of the fibres of the *uvea;* ſince this very dilatation of the pupil, compared with its coarctation ſome time after death, demonſtrates the truth of what I have advanced. But, after all, if the dilatation of the pupil be not owing to the elaſticity or natural contractility of the radiated fibres of the *uvea,* To what cauſe can it be aſcribed? For 'tis preſumed, our author has given up his notion of the aqueous humour preſſing the edges of the pupil outwards, as being contrary to the known laws of hydroſtatics. It may not, however, be improper to obſerve here, that, although we ſhould ſuppoſe the *uvea* to be, ſtrictly

a cordial julep, made the pupil inſtantly as wide as it is obſerved to be in a confirmed *gutta ſerena*; but ſoon after, it became narrower again; which is a ſufficient proof that the dilatation of the pupil, in this caſe, was owing to a contractile power communicated to the fibres of the *uvea,* by the *ſtimulus* of the volatile ſpirits or cordial; while its coarctation afterwards was only the conſequence of the *uvea* being more relaxed.

ſtrictly ſpeaking, not muſcular, but only a cellular membrane; yet, like the *dartos* of the *ſcrotum*, it would, by its elaſticity, retract the edges of the pupil as ſoon as the cauſe contracting it ceaſed to act. And although, at the time of death, the pupil would hence be rendered very large, yet ſome time after it, when this cellular ſubſtance began to loſe its elaſtic power, the pupil would become narrower.

M. de Haller, becauſe he cannot diſcover any orbicular muſcle ſurrounding the edge of the pupil, concludes there is none; and aſcribes the contraction of this part to a ſtronger influx of fluids into the ſmall veſſels of the *uvea*, occaſioned by the *ſtimulus* of light acting upon it. The inſufficiency of this hypotheſis we have ſhewn elſewhere *; and ſhall only add, that, as we conclude from the various motions of many of the ſmaller inſects, that they are, as well as larger animals, endowed with muſcles, though we can neither demonſtrate theſe inſtruments of motion by the anatomical knife, nor by the aſſiſtance of the microſcope, ſo we may infer the exiſtence of the orbicular muſcle of the *uvea* from the

---

* Eſſay on vital motions, p. 127. *&c.*

the regular motions of the pupil, although its texure may be ſo delicate as ſcarcely to be diſtinguiſhed by the anatomiſt from a denſe cellular membrane.

But to return; there are ſome other things advanced by our learned author, in his account of the irritable parts of the body, which, though not ſatisfactory, we ſhall paſs over *; and proceed to conſider what

---

* *M. de Haller* has repreſented me as ſaying, That the contraction of every muſcle of the body is interrupted with alternate relaxations *(a)*: whereas, in p. 20. 257. 260. and 261. of my Eſſay on the vital motions, I have expreſsly excepted the *ſphincter pupillæ*, muſcles of the internal ear, and ſome others, whoſe contraction is owing to a *ſtimulus* acting on ſome neighbouring or diſtant part. I have indeed affirmed, That thoſe muſcles to whoſe fibres a *ſtimulus* is immediately applied, are always agitated with alternate contractions and relaxations; and *M. de Haller* himſelf agrees with me in this, when he ſays, that all muſcles, not ſo much as one excepted, that he knows of, tremble and palpitate after death, and are alternately contracted and relaxed (Act. Gotting. vol. ii. p. 139. and 144). The bladder of urine, however, ſeems to differ from the other muſcles or muſcular organs in contracting uniformly and without any alternate relaxations, when it is pricked or expoſed to the cold air, in animals that are dying or

(a) *Act. Gotting.* vol. ii. p. 145.

what he has offered concerning the nature of irritability.

S E C T. II.

In my Eſſay on the vital and involuntary motions of animals, I had endeavoured to ſhew, that *ſtimuli* applied to the muſcles of animals excite them into contraction, by producing an uneaſy feeling in them or their nerves; but *M. de Haller*, who thinks irritability an innate property of muſcular fibres, contends, that it does not depend upon the nerves, and has no connection with ſenſibility:

1. Because the moſt ſenſible parts, ſuch as the nerves and ſkin, are not irritable*.

2. Because the irritability of our organs is obſerved to be in proportion to their ſenſibility †. And,

3. Because

---

or newly dead (*a*). In this reſpect, the bladder reſembles the *dartos ſcroti*, which is excited into a continued contraction, and furls up the *ſcrotum*, when cold water and aſtringent or acrid liquors are applied to it, or when the ſkin of the ſcrotum is gently irritated by titillation.

[*] Act. Gotting. vol. II. p. 134.

[†] Ibid. p. 136.

(*a*) Act. Gotting. vol. 2. p. 142. and 145.

3. Because parts deſtitute of feeling are irritable *.

With regard to the firſt of theſe, ſince muſcles are the only organs of the body which, by their particular fabric, are fitted for motion, it is ſo far from being a wonderful diſcovery, as our author ſeems to think, That the nerves are deſtitute of irritability, that it is only a neceſſary conſequence of their make; for a power of contraction does not depend on ſenſibility alone, but upon this in conjunction with a particular ſtructure.

The proper anſwer therefore to this firſt argument is, That, altho' irritability always infers ſome degree of ſenſibility, yet ſenſibility does not infer irritability, unleſs the part be, by its peculiar fabric, fitted for motion, *i. e.* in other words, unleſs it be what we call muſcular †.

---

* Act. Gotting. vol. II. p. 134.

† Notwithſtanding what is ſaid here to ſhew that ſenſibility does not infer irritability unleſs in parts which are muſcular, *M. de Haller* has accuſed me as being guilty of a groſs inconſiſtency, when I ſay irritability is proportional to ſenſibility, becauſe I acknowledge the nerves to be deſtitute of this power.

Altho' the ſkin is not irritable in the ſame ſenſe that the muſcles are, yet the inflammation and pain raiſed in it by bliſters and other acrid applications ſhew, that it is very readily fretted or irritated by *ſtimuli*. The ſkin, when ſtimulated, is not brought into alternate contractions, becauſe it is not by its ſtructure made capable of this kind of motion; but it becomes red, is inflamed, and pours forth its liquors ſo copiouſly, as to ſeparate the ſcarfſkin, and raiſe it in the form of a bladder filled with water, becauſe the ſmall veſſels, of which it is in a great meaſure compoſed, partake of a muſcular nature, and are, like the larger muſcles, excited into alternate contractions by *ſtimuli*.

Further, the *dartos* or cellular membrane of the *ſcrotum* is contracted uniformly, when expoſed to the cold air or other *ſtimuli*; and the ſkin, from the application of cold air or water, ſeems likewiſe to ſuffer ſome kind of contraction, by which means

---

See *Memoires ſur les parties ſenſibles*, &c. vol. iv. p. 118. There is no inconſiſtency in what I have advanced on this head; and ſurely nothing but inadvertency could have been the occaſion of *M. de Haller*'s miſrepreſenting me in the manner he has done.

means it is raiſed into tubercles reſembling the ſkin of a gooſe. When cold water is ſuddenly, and without one's knowledge, applied to a part of the body that is warm, there is excited inſtantly a kind of ſhivering which ſpreads over the whole body; and not only the pores of that part to which the cold water was applied, but alſo of the whole body, are contracted. Do not theſe examples ſhew that the *dartos* and ſkin are affected by *ſtimuli*, and conſequently irritable, though not in the ſame ſenſe that the muſcles are? The irritability of the parts of the human body, therefore, may perhaps be not improperly diſtinguiſhed into three kinds: *viz*. That power of alternate contraction which is peculiar to thoſe organs we call muſcles; that uniform conſtriction which happens to the *dartos* and pores of the ſkin; and that redneſs and inflammation which is excited in every part of the body that is ſenſible, as often as acrid things are applied to it; although indeed this laſt is only an effect of the firſt kind of irritability in the ſmall veſſels of the parts.

As to the ſecond argument, *viz.* That irritability is not obſerved to be in proportion to ſenſibility, our author has been very unlucky; ſince an inflammation of any irritable organ, which increaſes its ſenſibility, is always obſerved to make it more irritable, as will be ſhewn afterwards by a variety of examples. The Doctor, however, in proof of his aſſertion, tells us, that the ſtomach is more ſenſible than the inteſtines, and yet leſs irritable; and that the heart itſelf is endowed with no acute feeling, and, when touched in a living perſon, occaſions fainting rather than pain *.

THE ſtomach has a particular feeling whereby it is very diſagreeably affected by things that, as far as we can judge by our taſte or ſmell, have very little acrimony: it is the principal ſeat of hunger; and, as when we have wanted food for any conſiderable time, it is affected with a more diſagreeable ſenſation than the guts, ſo likewiſe it is more ſenſible of an agreeable feeling from grateful food: and in theſe reſpects, it may be ſaid to be more ſenſible than the inteſtines. But, notwithſtanding this,

---

* Act. Gotting. vol. II. p. 136.

this, the inteſtines ſeem to be as ſuſceptible of pain as the ſtomach, or indeed, any other organ of the body; an inflammation in them is as painful, if not more ſo, than in the ſtomach; and jalap, *ſenna*, and other ſmart purgatives, which ſeldom occaſion any pain in the ſtomach, often affect the guts with ſevere gripings.

With regard to the heart; Dr *Harvey* ſeems too haſtily to have concluded it to be void of feeling, becauſe the young Nobleman whoſe heart he touched ſcarce felt any thing at all; for what this great man put his fingers to, was not the ſubſtance of the heart itſelf, but an inſenſible *callus* covering and defending it *. The truth of the

---

* Becauſe a teſticle covered with a *callus* or fungous ſubſtance is ſometimes very ſenſible when touched, *M. de Haller* ſeems to think that the ſmall degree of feeling in the heart of that nobleman mentioned by *Harvey* could not be owing to its callous covering (*a*). When a part which is covered with a *callus* is inflamed, it will doubtleſs be painful when touched; nay, preſſing even the nail gives pain when the nerves below it are inflamed: But to ſay in general that any part ſhould be redered more ſenſible by its being ſurrounded with a *callus*, is ſuch a paradox as it would be to affirm that

(*a*) Element. Phyſiolog. vol. 1. p. 489.

the matter is, that, as the ſkin, although one of the moſt ſenſible parts of the body, feels no pain from a ſlight preſſure or attrition, becauſe it is defended by the inſenſible *epidermis;* ſo the heart, when gently touched, feels little, becauſe it is covered with the inner *lamina* of the *pericardium*, which, like other membranes of the body, has but a ſmall degree of ſenſibility *. In like manner, the external ſurface of the inteſtines is rendered leſs ſenſible than it would otherwiſe be, by their being involved in the meſentery; and hence it is, that the woman mentioned by *Peyerus* felt no pain when her inteſtines were handled by him and *Wepferus* †. But, although the outer ſurface of the heart and inteſtines may have no great degree of ſenſibility, it will not thence follow, that their internal ſurface, where the natural *ſtimuli* exciting their motions act upon them, is not endowed with a more exquiſite feeling: nay the contrary is highly probable, if not altogether

that the fingers of a blackſmith feel more acutely than thoſe of a delicate woman.

* Act. Gotting. vol. II. p. 130.

† Parerg. anatom. exercitat. I. cap. iv.

together certain. *M. de Haller* himſelf has obſerved, that the heart is much more affected in animals dying, or newly dead, by the gentle *ſtimulus* of warm water or air puſhed into its ventricles, than by applying the moſt acrid liquors to its external ſurface, or even pricking it with the point of a knife *; and it will appear from an experiment to be mentioned afterwards, that, in ſome caſes, the *ſtimulus* of the blood within the cavities of the heart will excite a tremulous motion in this organ, when oil of vitriol applied to its external ſurface has not the leaſt effect this way.

WITH regard to the comparative ſenſibility and irritability of the heart and inteſtines, it is not eaſy to ſay any thing certain; nor is this needful; ſince from our author's experiments it does not appear clearly, whether the heart or inteſtines are moſt irritable †. The motions of the heart are indeed ſtronger and more frequently repeated; but thoſe of the inteſtines continue, in many animals, as long, if not longer, after death.

As

---

* Act. Gotting. vol. I.

† Act. Gotting. vol. II. p. 147.

As for our author's third argument, *viz.* that parts deſtitute of feeling are irritable; there is not ſo much as one inſtance given, nor indeed can be given, of a part being irritable that is naturally inſenſible and deſtitute of nerves*: but what he thinks equivalent to this, is, that muſcles continue to be irritable, not only for ſome time after their nerves have been tied or cut, and ſo all communication between them and the brain intercepted, but alſo after they have been intirely ſeparated from the body. And, indeed, it muſt be owned, there is a great deal of ſeeming weight in this argument: but that it is, neverthleſs, inconcluſive, has been already ſhewn in the laſt ſection of my *Eſſay on the vital and other involuntary motions of animals*; and will, I hope,

appear

---

* Our author indeed mentions, upon the authority of *Lupſius*, the ſecundines and membranes of the *ovum* as irritable, and yet deſtitute of nerves. But, if irritability, as he himſelf allows, be a property of muſcular fibres alone, it will follow, that the membranes of the *ovum*, which are not muſcular, cannot be irritable: but, ſuppoſing they were both the one and the other, it is not a clear point, that they may not be ſupplied with ſmall nervous filaments propagated to them by means of the navel-ſtring.

appear ſtill more ſo from the following conſiderations.

1. ALTHOUGH the irritability of muſcles continues, in a ſmall degree, for ſome time after their nerves are tied or otherwiſe deſtroyed; it will not follow, as our author thinks, that this power does not depend upon, or proceed from the nerves: for, if this were ſo, one would expect that, in a living animal, where the muſcles are all ſupplied with blood by the arteries, they ſhould continue to preſerve their power of irritability, not only for a few minutes, but for many hours and days after their nerves have been tied or cut. Further, if the irritability of the muſcles were not owing, ſome how, to the nerves or their influence, why ſhould an irritation of the nerves or *medulla oblongata*, produce ſuch remarkable convulſions?

THESE convulſions cannot be owing to the propulſion of any ſubtile fluid in the nerves towards the muſcles; ſince, as *M. de Haller* and others have obſerved *, theſe motions follow equally whether a nerve going to any muſcle is ſqueezed upwards or

---

* Act. Gotting. vol. II. p. 136.

or downwards. If they were owing to the connexion or vicinity of the nerves to the muſcles, one would expect, that more remarkable convulſions ſhould follow from an irritation of the tendons than of the nerves: the contrary of which, however, is true; for, while the irritation of a nerve produces ſtronger convulſive motions in the muſcles, than ariſe even from the laceration of their own fibres, the tendon, however pricked or irritated, produces no change in them *. The reaſon is plain; the tendon has little or no feeling, while the nerves has a very acute one.

BUT further, it ought to be obſerved, that when, after decollation, a frog's ſpinal marrow is deſtroyed with a red hot wire, no viſible motion is produced in its limbs or body, by pricking, cutting, or otherwiſe hurting them: only, when the ſkin of the thighs was diſſected off, and the muſcles were irritated, their fibres were agitated with a weak alternate tremulous motion. Now, as the ſtrong convulſive motions excited by irritation in the legs and trunk of the body of a frog after decollation,

* Act. Gotting. vol. II. p. 140.

collation, are certainly to be aſcribed to the intire ſtate of the ſpinal marrow, ſince they ceaſe as ſoon as it is deſtroyed; Is it not highly probable, that the weak tremulous motion in the irritated muſcles of a frog's thighs, after the deſtruction of the ſpinal marrow, were owing to the influence or power of their nerves, which ſtill remained intire *? It ſeems alſo to deſerve particular notice, that, after the deſtruction of the ſpinal marrow, altho' the fibres of ſuch muſcles as were irritated exhibited a weak tremulous motion; yet there was no ſympathy between the different muſcles, or other parts of the body, as was obſerved, while the ſpinal marrow was intire: from whence it ſeems to follow, that the nerves diſtributed to the ſeveral parts of the body have no communication but at their termination in the brain or ſpinal marrow; and that to this, perhaps alone, is owing

---

* As the alternate motions of the heart, in many animals, continue for a long time after the deſtruction of the brain and ſpinal marrow; Is it not probable, that its nerves are ſo conſtituted as to make its moving power leſs dependent on immediate ſupplies from the brain and ſpinal marrow, than that of the voluntary muſcles?

owing the conſent or ſympathy obſerved between them.

Upon the whole; the weak alternate motions produced by irritating muſcles whoſe nerves have been tied or cut, by no means prove, that their irritable power is independent of the nervous influence: they only ſhew, That theſe motions are not owing to any new derivation of ſpirits from the brain into the muſcles at that time----- That the preſence of the nervous influence in their fibres is only requiſite; and that the ſpirits remaining in the nerves, below the ligature and in the muſcular fibres, may be ſufficient to preſerve a certain degree of irritability, or power of motion in them, for ſome little time.

M. de Haller further concludes irritability to be independent of the brain and nerves; becauſe the ſmalleſt inſects, which have no head, are irritable *: but, if this argument was good for any thing, it would prove ſenſibility and voluntary motion to be alſo independent of the brain and nerves; for the ſmalleſt inſects ſeem to be endowed with feeling, and undoubtedly perform

---

* Act. Gotting. vol. II. p. 156.

perform voluntary motions. May not theſe inſects which want a head have ſomething to ſupply the place of a brain, from which the nerves may take their riſe? Or may not the nerves be ſo formed in them, as to be ſufficient of themſelves, without a brain, for the purpoſes of motion and ſenſation? Arguments of this kind, which are drawn from our ignorance of the true ſtructure of animals, can be of no weight.

2. M. DE HALLER, while he denies feeling to the *dura* and *pia mater*, allows it to the medullary ſubſtance of the brain *; becauſe, when it is wounded, the muſcles of the body are convulſed in an extraordinary manner. Now, if the ſenſibility of the medullary part of the brain in living animals may be deduced from the convulſive motions which enſue upon hurting it, Are we not (our author himſelf being judge) to aſcribe feeling to the brain, even in animals newly killed; ſince in theſe, the motion of the heart is renewed by irritating the *medulla oblongata*, and the whole muſcles of the body are convulſed by diſſecting

---

* Act. Gotting. vol. II. p. 130. et 134.; et Primæ lineæ phyſiolog. 2d edit. p. 238.

ing the ſpinal marrow *? And altho', in animals newly dead, the convulſive motions produced by irritating the *medulla oblongata* or *ſpinalis* be weaker and leſs remarkable than in living animals; yet it will by no means follow, that they are not indications of ſenſibility, and owing to the ſame cauſe as in living animals: for, as the death of the body, in general, ſoon puts an intire end to every kind of feeling and activity in the parts of moſt animals, ſo it is not to be doubted, that, immediately after death, theſe powers begin to be weakened; wherefore the motions owing to them muſt be leſs remarkable.

Further, if the convulſions occaſioned by irritating a nerve in its natural ſtate are allowed by all to be a proof of its feeling, the like, tho' weaker, convulſions excited in the muſcles by irritating a cut or tied nerve muſt be an equal proof of its ſtill retaining, in ſome meaſure, its ſenſibility. When all communication, therefore, with the brain by means of the nerves, is cut off, convulſive motions, which ariſe from a *ſtimulus* applied to any part, are equally

---

* Kauu Impet. faciens, No. 330. et 333.

a proof of the ſenſibility of that part as if the communication were preſerved. And, if in the latter caſe, theſe motions may be juſtly aſcribed to the nerves, being hurt by the irritation, they muſt be equally ſo in the former.

3. BUT here it will be objected; How can there be any ſenſibility or feeling in a nerve whoſe communication with the brain it cut off?

IN anſwer to which, it may be ſufficient to ſay, That, ſince we have ſtrong reaſons for believing that the parts of many inſects continue to be ſenſible for a conſiderable time after they have been divided from each other *; and that the bodies of ſome larger animals continue to live and feel after they are deprived of their heads †: Why may not the muſcles of men preſerve ſome degree of ſenſibilty for a few moments after

---

* Flies copulate and lay eggs after decollation; *Boyle*'s Uſefulneſs of experimental philoſophy, part 2. p. 16.

† Vipers continue for three days after being deprived of their head and heart, to be manifeſtly ſenſible of punctures, and move their bodies, when pricked, juſt as intire vipers do; *Boyle*'s Uſefulneſs of experimental philoſophy, part 2. p. 16.

ter their nerves are tied or cut, although we may not be able to account for this, from any thing we know of the nature of the body, or of the manner in which the ſoul is preſent with, or acts upon it *?

---

* If I were allowed to indulge myſelf in conjectures concerning a matter of which I know very little, I would ſay, that, although there can be no feeling or perception in the brain when a nerve is pricked below where it is cut or tied; yet, if the ſoul be preſent every where in the body, as ſeems highly probable, there may be ſome kind of feeling or ſenſation excited in the nerve itſelf, which may be ſufficient to produce a motion in the muſcles to which it belongs.

Dr *Stewart* has produced ſeveral arguments to prove, that the inferior extremity of every nerve is to be conſidered as the brain of the organ or part in which it terminates; and that the ſoul is not confined to the brain or any part of it, but is preſent every where in the body, equally in the extremities of the nerves, as at their origin; (Diſſert. de motu muſcular. cap. v.) And if this be ſo, as it may for any thing that can be ſhewn to the contrary, why may not a muſcle, whoſe nerve is tied or cut, continue, for ſome little time, ſenſible and irritable? Its ſenſibility will not indeed be attended with what is properly called *conſciouſneſs*, as diſtinguiſhed from *ſimple ſenſation;* becauſe this reflex act, by which a perſon knows his thoughts or ſenſations to be his own, is a faculty of the ſoul only exerciſed in

REDI tells us, that the head of a viper will bite half an hour after it is cut off from the body, (*Vid. Jacobæi Obſervat. de ranis et lacert. p.* 58.); and I have often obſerved, that

the brain, with which all communication is now cut off.

As the ſoul ſeems to imagine, judge, reaſon, and remember in the brain only; ſo, why may it not have, in the various other parts of the body, ſuch feelings or powers as are neceſſary for carrying on their ſeveral functions? Particularly, why may it not, in the muſcular fibres, have the power of ſimple ſenſation and of beginning motion? Or, which will amount to much the ſame thing, while the rational ſoul acts only in the brain, there may perhaps be, as ſome have thought, a ſentient active principle, which enlivens the whole body, and which continues to actuate the parts, for ſome time after their communication with the brain is ſtopt, *i. e.* as long as they continue in due order for being acted upon by it.

The more probable opinion, however, ſeems to be, that the ſoul is equally preſent in the extremities of the nerves through the whole body as in the brain. In thoſe, it is only capable of feeling or ſimple ſenſation; but in this, it exerciſes the powers of reflex conſciouſneſs and reaſon. When the communication of any part with the brain is cut off, the ſimple ſenſation of feeling excited in ſuch part is no longer perceived by the ſoul in the brain; and therefore is not attended with reflex conſciouſneſs: the nerves being now alſo ſeparated from

that a frog's head, after being ſeparated from its body, not only continued, for above half an hour, to move its eye-lids, noſtrils, and muſcles of the lower jaw,

O when

from the brain, ſoon become unfit to perform their functions; hence the powers of ſimple ſenſation and motion in the part, if it be muſcular, ceaſe by degrees, till at laſt it becomes quite dead. The communication, therefore, between the ſeveral organs and the brain, is not only neceſſary to preſerve their nerves, by means of ſome influence tranſmitted to them in due order for performing their functions and being properly affected by their ſeveral objects, but alſo, that the ſoul, as a conſcious and rational being, may be acquainted with theſe impreſſions.

It will be unfair to object here, that we aſcribe the intelligent powers of the mind to the bodily organs: for as the beſt muſician cannot make a flute give the ſound of a violin, nor a harpſicord that of a French horn, nor without theſe ſeveral inſtruments produce their ſounds and notes at all; in like manner, the ſoul, in the *preſent* ſtate, can only exerciſe its rational powers in the brain; it can only taſte in the tongue, ſmell in the noſe, ſee in the eyes, hear in the ears, and feel hunger in the ſtomach. But although the imagination, memory, and rational powers, depend upon the brain; yet the brain does not imagine, remember, or reaſon: although taſte depends on the tongue, ſmelling on the noſe, ſeeing on the eyes, and hearing on the ears; yet theſe

when its brain or the ſkin of its head was touched with a probe, but ſometimes moved its eyes and eye-lids, when nothing touched it, and as it were of its own accord; ſo that, without an obſtinate degree of ſcepticiſm in this matter, we cannot deny that the head continues to be animated for a conſiderable time after it is ſeparated from the body, and to perform not only involuntary motions when ſtimulated, but, in appearance, alſo voluntary ones. In like manner, the body of a frog, after being divided from the head, preſerves the power of motion for above an hour; and when its hind feet or toes are cut, or otherwiſe hurt, the muſcles of its thighs, legs, and trunk are ſtrongly contracted, by which it raiſes its body from the table, and ſometimes moves from one place

---

theſe organs neither taſte, ſmell, ſee, nor hear, but only that living ſentient principle which animates them.

It may be proper to obſerve, that, whether theſe conjectures, which are offered with a great deal of diffidence, ſhall be thought probable or not, the argument concerning the irritable power of the muſcles of animals will not be materially affected; ſince this muſt be determined, not by metaphyſical reaſonings, but by experiments and obſervations. Vid. ſect. iv. below.

place to another. When the muſcles of the thighs are pricked or cut with a knife, they are excited into contraction; but neither they, nor the neighbouring muſcles, are near ſo ſtrongly convulſed as when the toes are wounded: Whence ſhould this happen; and why ſhould not the muſcles of the legs and thighs be more ſtrongly convulſed, when they themſelves are wounded, than when the toes are treated in the ſame manner? This would undoubtedly be the caſe, if the motions of irritated muſcles were owing to ſome property of the inſenſible matter compoſing them. But if, as we imagine, they are all to be derived from feeling, it is eaſy to ſee, that, as the feet and toes are more ſenſible of pain when wounded, than the muſcles of the legs or thighs, ſtronger convulſions muſt be occaſioned by an irritation of the former than of the latter.

Further, we muſt either allow that both the head and body of a frog continue to be animated, for a conſiderable time, after they are ſeparated from each other; or elſe affirm, that the life, feeling, and active

powers

powers of animals, are merely properties of that kind of matter of which they are compoſed. The former opinion is attended with ſome difficulties, which ariſe *ſolely* from our ignorance of the nature of immaterial beings: the latter is inconſiſtent with all that we know of matter or its properties. If we admit it, therefore, we not only aſcribe qualities to matter which it does not poſſeſs, but preſume to limit, by our ſcanty and inadequate capacities, the powers of incorporeal natures, their manner of acting upon bodies, and co-exiſting with them.

If the ſoul were confined to the brain as many have thought [*], Whence is it that a pigeon not only lives for ſeveral hours after being deprived of its brain, but alſo flies from one place to another [†]? And to what cauſe are we to aſcribe the continuance of life and motion in a viper for three days after its head is cut off, and in a tortoiſe for three weeks after decollation, and ſix months after the loſs of its brain [‡]? The motions

---

[*] Act. Gotting. vol. ii. p. 153.

[†] Baglivi opera, 4to. præfat. p. xi.

[‡] Redi, Obſervat. circa animal. vivent. p. 209. &c.

motions performed by theſe animals cannot ſurely be attributed to their material part alone; unleſs we ſhall deny them a ſoul altogether, and, with *Des Cartes*, refer all their actions to their corporeal machinery. The late Reverend and ingenious Dr *Hales* informed me, that having, many years ſince, tied a ligature about a frog's neck, to prevent any effuſion of blood, he cut off its head, and, thirty hours after, obſerved the blood circulating freely in the web of the foot: the frog alſo at this time moved its body when ſtimulated: but, on thruſting a needle down through the ſpinal marrow, the animal was ſtrongly convulſed, and, immediately after, became motionleſs.

If then the ſoul in pigeons, frogs, vipers, and tortoiſes, is by no means confined to the brain, but can continue for a long time to actuate their bodies independent of it; and if, in may inſects which have no brain, every part of the body is both ſenſible and irritable *; why ſhould we deny, that, in man and ſuch animals as reſemble him moſt, the parts may continue to be actuated

---

* Act. Gotting. vol. ii. p. 138.

ted by the ſoul or ſentient principle for ſome few minutes after their communication with the brain has been cut off *?

If any man of ordinary ſenſe, who is no philoſopher, be aſked, Why the heart of a frog beats after being ſeparated from the body, and renews its motions when pricked? he will readily ſay, It is becauſe there is *life* in it: and this is a proper anſwer; nor can a better, perhaps, be given by the ableſt philoſopher. If then *life* in animals be owing to the energy of a principle diſtinct from matter, and of powers ſuperior to it, we have reaſon to conclude, that, as long as any ſigns of life remain in the bodies of animals, or any of their parts, this principle ſtill continues to actuate them.

There are two kinds of motion from irritation, obſervable in living animals: *viz.* where the muſcle or organ itſelf is ſtimulated;

---

* The difference between men and thoſe animals which live long after decollation or the exciſion of their heart, ſeems to be, that the latter are ſo framed that freſh ſupplies of blood and ſpirits from the heart and brain are not immediately neceſſary to keep the ſeveral parts in due order to be acted upon by the ſoul: as ſeems to be, in a great meaſure, the caſe in man and many other animals.

mulated; and where the *ſtimulus* only affects ſome neighbouring or diſtant part. The firſt (of which kind is the motion of the heart) ſeems to be owing to the ſoul or ſentient principle as acting in the part moved; but the ſecond, to the ſoul as perceiving and acting in the brain: and of this kind is the motion of ſneezing from an irritation of the noſe, and the contraction of the diaphragm in vomiting and in a *teneſmus* or ſtrangury. In order to the firſt kind of motions, an immediate communication with the brain is not abſolutely neceſſary, but only ſuch a ſhare of the nervous power in the muſcle or its nerves, as may be requiſite to fit its fibres for being acted upon by the ſoul or ſentient principle. But the caſe is quite otherwiſe in the ſecond; where the motion produced is through the intervention of the brain, and not by any *ſtimulus* applied to the part moved. And hence it is, that, in an animal newly dead, the diaphragm is not brought into contraction by lacerating or pricking the *inteſtinum rectum* or neck of the bladder, altho' the fibres of theſe parts themſelves

may be, thence, agitated with ſome tremulous motions. In like manner, though the muſcular coat of the ſtomach is excited into contraction ſome time after the death of the animal by irritating it; yet the diaphragm is no ways affected by this irritation: which, however, it would have been, if the animal had been alive. Agreeably to this, when any of the muſcles of a frog's legs are irritated ſome time after cutting off its head, almoſt all the muſcles belonging to the legs and thighs are brought into contraction, if the ſpinal marrow be entire: but, as ſoon as this marrow is deſtroyed, although the fibres of ſuch muſcles as are themſelves ſtimulated are affected with a weak tremulous motion, yet the neighbouring muſcles remain altogether at reſt.

I HAVE elſewhere endeavoured to ſhew, That the ſuppoſition of the ſoul or ſentient principle's continuing for ſome time to actuate the ſeparated parts of animals, does not infer its *real* diviſibility *; nor is it neceſſary to repeat the ſame things again: but, I cannot help obſerving, that, when

M. de

---

* Eſſay on vital motions, &c. p. 380. &c.

M. de Haller repreſents me as holding the ſoul to be diviſible, ſo as that it may be cut into as many pieces as the anatomiſt pleaſes *; he charges me with an opinion which I not only do not maintain, but which I have brought arguments to diſprove. I ſhall only add, that the indiviſibility of the ſoul does not depend on the unity of the body, but on its own particular nature.

It muſt be acknowledged, that there is a great deal of obſcurity in theſe matters: but as in every part of nature we find abundance of myſteries, as often as we puſh our inquires to any great depth; it can be no wonder if we meet with difficulties, almoſt inſurmountable, in accounting for the motions of animals, or tracing them up to their firſt ſource: for, if we are far from underſtanding the communication of motion and other actions of *matter* upon *matter*, How ſhall we be able to comprehend the manner in which an *immaterial* principle acts upon it? But, as we can, from the little we know of matter, ſee that inactivity is one of its eſſential properties, we are hence convinced

---

* Act. Gotting. vol. II. p. 137.

convinced of the neceſſity of aſcribing the life and motions of animals to the power of an *incorporeal* agent.

If we knew the manner of exiſtence of the ſoul, or the way in which it acts upon, or is preſent with the body; it would be a very proper objection to any phyſiological opinion, that it was inconſiſtent with what we certainly knew of theſe things: but, as we are utterly ignorant of them, it is highly unreaſonable and abſurd to argue againſt an opinion ſupported by experiment and analogy, from its ſuppoſed inconſiſtency; with what? why, truly, with nothing! For what we are totally ignorant of, is, to us, as if it were nothing; and we can neither affirm nor deny any thing to be either conſiſtent or inconſiſtent with it.

SECT. III.

M. de Haller, after endeavouring to prove that irritability is independent on ſenſibility, gives it as his opinion, That this remarkable property of the muſcles has its ſeat in the glutinous matter connecting the

the earthy elements of which their fibres are compoſed *; and that irritability ought to be looked upon as a particular property of this glutinous ſubſtance, in like manner as gravity is allowed to be a property of matter in general, altho' its cauſe cannot be aſſigned †.

But ſurely the glutinous matter of the muſcles of animals ſeems as unlikely to be endowed with an active power, ſuch as irritability, as any other conſtituent part of the animal body; nor can any thing be deduced from its endeavouring to ſhrink or ſhorten itſelf when drawn out ‡; for the glue of the ſkin, ligaments, and tendons, as well as of the muſcles, has this property, which is, indeed, a kind of elaſticity ||, and no way ſimilar to that power of alternate contraction which muſcular fibres are endowed with.

The Doctor, in proof of his notion of the

---

* Act. Gotting. vol. II. p. 152.

† Ibid. p. 154. and 157.

‡ Ibid. p. 152.

|| Elaſticity is not a property of hard bodies alone, as *M. de Haller* ſeems to think (p. 152.), but is alſo found in ſoft ones: thus air, wool, and the down of feathers are remarkably elaſtic.

the irritable nature of the muſcular glue, adds, that young animals which abound moſt in it are moſt irritable. The obſervation is certainly true, but proves nothing in the preſent caſe; for the ſkin, ligaments, and tendons (which laſt are a continuation of the muſcles, only harder and more compacted) abound much more in *glue* than the muſcles, and yet are not in any degree irritable. The greater irritability of the fibres of young animals is to be deduced from their greater ſenſibility, and this is owing to their greater ſoftneſs and tenderneſs: thus, what in new-born animals is a ſenſible and irritable muſcle, becomes afterwards a tendon, which, in a ſound ſtate, is deſtitute of irritability, and endowed with little or no feeling *.

But further, ſince the gelatinous matter in our aliments, and even in our blood, is quite deſtitute of the property of irritability, it muſt owe this power to the particular diſpoſition or arrangement of its parts, or to ſome other change which it ſuffers, when it becomes a part of a muſcle: And if this may be ſo, why may not the

---

* Act. Gottingenſ. vol. II. p. 140.

the finer and more ſubtile parts of the blood be ſo changed in the brain, as there to acquire a power of feeling and thinking? *i. e.* if *irritability* be a poperty of the muſcular *glue,* why may not ſenſibility and intelligence be properties of the medullary ſubſtance of the brain? for the known properties of matter give us reaſon to think, that real activity is not more conſiſtent with its nature, than feeling or thought.

BUT it has been ſaid, that irritability may be a property of the muſcular glue, as well as gravity is a property of matter in general: let us therefore conſider this notion a little, and ſee whither it will lead us. Gravity, which is a property of matter, continues to be ſo, let matter be ever ſo much altered or changed by fire, menſtruums, or other cauſes; but, when the *gluten* of the muſcles is extracted from them, it appears as inert and deſtitute of active powers as any other matter; nay, tho' allowed to remain in them, yet, in moſt animals, it loſes its power altogether very ſoon after the muſcles are ſeparated from the body.

But, ſuppoſing irritability to be a property of the muſcular glue, in the ſame ſenſe that gravity is a property of all matter; yet, as the moſt attentive conſideration of the nature of matter has convinced philoſophers that gravity is not eſſential to it, but owing to ſome general cauſe acting upon it; ſo the irritability of the muſcular glue muſt be allowed not to be a property eſſential to it, but ariſing from the action of ſome other cauſe upon it. Gravity has been aſcribed either to the immediate and continued operation of an *immaterial* being, or to the action of ſome ſubtile elaſtic *medium* on matter: But, ſince the elaſticity of the parts compoſing ſuch a *medium* muſt be, at laſt, referred to the active power of ſome *incorporeal* cauſe, it follows, that gravity muſt be ſo likewiſe *.

It

---

* *M. de Haller* has repreſented me as maintaining an error contrary to the *common notions* of mankind (*a*), when I ſay, that gravity, or rather the elaſticity of that *medium* ſuppoſed to be the cauſe of gravity, muſt be, at laſt, referred to the active power of ſome incorporeal cauſe. How far we ſhould be ſafely directed in our reaſonings concerning the nature of cauſe

(*a*) *Memoires ſur les parties ſenſibles*, &c. vol. iv. p. 91.

It appears, therefore, after all that has been ſaid to ſhew that the motions of irritated muſcles are owing to a property of irritability in them or their glue, that we are at laſt obliged to refer them to the active power of an *immaterial* cauſe; unleſs we ſhall, contrary to all ſound philoſophy, chuſe to aſcribe feeling and proper activity to matter. And, as *gravity* muſt finally be reſolved into the power of that Being who upholds univerſal nature; ſo it is highly probable, that the irritability of the muſcles of animals is owing to that living ſentient principle which animates and enlivens their whole frame.

---

cauſe of gravity by the common notions of mankind, I ſhall leave the reader to determine; but ſome very able philoſophers have thought, and not without reaſon, that gravity, or the material cauſe producing it, muſt at laſt depend upon the power of that Being who ſuſtains, moves, and governs the whole ſyſtem of nature: and *M. de Haller* will find it a hard taſk to account for the elaſticity of that æthereal *medium* ſuppoſed to be the cauſe of gravity, from any thing we know of the nature of matter, or of the properties that are eſſential to it.

SECT. IV.

Having thus endeavoured to lay open the inſufficiency of *M. de Haller*'s theory of irritability, I ſhall conclude with a few obſervations, which, if they do not demonſtrate, make it at leaſt extremely probable, that the motions of ſtimulated muſcles proceed from their ſenſibility, or are cloſely connected with it. But, previous to theſe, I muſt be allowed to take notice, that the word *irritability* ſeems to imply a kind of life or feeling in the part endowed with it, which renders it capable of being fretted, provoked, or irritated; and therefore ſeems to be improperly applied to expreſs the contractile power of ſtimulated muſcles, if this power has no connection with, or dependence on their ſenſibility *. We never talk of irritating a ſtone, a piece of wood, a tree, or indeed any thing that is

---

* *Quicquid incommodum, alicunde illatum percipit, idemque a ſe amoliri ſatagit, id proprie dixeris irritari. Idcirco, cum partes iſtæ injurias perſentiſcant, ſeſeque ab iiſdem vindicare conentur, irritationis quoque capaces merito dicendæ ſunt; Gliſſon. Anatom. hepat.*

is deſtitute of feeling. *Irritability,* therefore, in the common acceptation of the word among mankind, implies ſome kind of feeling; nay, *M. de Haller* himſelf, notwithſtanding his profeſſed deſign is to ſhew irritability to be independent of ſenſibility, ſpeaks once and again of parts that are not irritable, as not feeling or perceiving the acrid matter, or other *ſtimulus* applied to them *. So true is the obſervation of the poet,

> *Naturam expelles furca; tamen uſque recurret.*

But to return;

I. We almoſt always obſerve the irritability of the muſcles or muſcular organs of the human body to bear a proportion to their ſenſibility. Thus, in young children, where the tender nerves and fibres are more eaſily hurt, and all the feelings are more exquiſite; the quickneſs of the pulſe and the violent convulſions with which they are often affected, from very ſlight cauſes, ſhew their muſcles to be endowed with a greater degree of irritability, than those

---

* Acta Gottingenſ. vol. ii. p. 142.

thoſe of adults *. In like manner, grown people of delicate nerves and very quick feelings are ſubject to ſpaſms and convulſive motions of their ſtomach, inteſtines, *&c.* and to palpitations of their heart, from ſuch ſlight cauſes as would ſcarce ſenſibly affect men of firmer conſtitutions and leſs moveable nerves.

On the other hand, in old people, in whom all the feelings become leſs acute, the muſcles are leſs irritable; witneſs the ſlow motion of their heart. And, in apoplectic and comatous caſes, where the ſenſes are greatly impaired, the heart's motion, and that of reſpiration are remarkably ſlow; and the *ſtimulus* of the *fæces* is not ſufficient to bring the inteſtines, diaphragm, and abdominal muſcles into contraction as uſual.

Further, the nerves, which are the moſt ſenſible parts of the body, produce, when irritated, the moſt remarkable convulſive

---

* It may alſo be obſerved, that the parts of young animals which are moſt ſenſible, are not only moſt irritable, but retain their power of motion longeſt after death, or ſeparation from the body. Vid. Eſſay on vital motions, p. 358.

vulſive motions in the muſcles; and, when they are, by being ſtretched, rendered more ſuſceptible of pain, an irritation of them produces ſtill greater convulſions *.

II. WHATEVER increaſes the ſenſibility of the muſcles or moving organs of our body, alſo increaſes their irritability.

THUS, when the ſtomach is inflamed, the mildeſt liquors received into it are apt to provoke vomiting, or the hiccup; whereas, in a ſound ſtate of this organ, brandy, vinegar, and other acrid liquors, produce no ſuch effect. When the neck of the bladder is ſlightly inflamed or excoriated, the urine, which uſed to give little diſturbance till collected in large quantity, irritates this tender part, ſo as to produce violent and often repeated efforts to empty the bladder.

WHEN the *fauces* are attacked with an inflammation, the muſcles of deglutition are more ſtrongly convulſed in ſwallowing, than when theſe parts are in their natural ſtate. When the inteſtines are by any means deprived, in a good meaſure, of their *mucus*, or rendered more ſenſible by

---

* Act. Gottingenſ. vol. ii. p. 136.

a very ſlight degree of inflammation in their inner membrane; the gentleſt purgatives often operate as ſeverely as the ſtronger ones do in a perſon in perfect health. When, without any degree of erection in the *penis*, the *ſemen* eſcapes into the *urethra*, the *muſculi acceleratores urinæ* are no ways affected by it: but, as often as the *penis* is erected, and thereby its parts rendered more ſenſible, and, as it were, half inflamed, the *ſemen* is no ſooner poured into the beginning of the *urethra*, than the above mentioned muſcles are excited into ſtrong convulſive contractions.

The heart becomes ſo irritable, when itſelf or the *pericardium* is inflamed, as to be agitated with violent convulſions and palpitations. Nay, the tendons, which, in a ſound ſtate, have little or no feeling, and are not irritable *, become, when inflamed, ſo ſenſible of *ſtimuli*, that the moſt violent convulſions have been occaſioned by pricking, tearing, or otherwiſe irritating them.

A diſagreeable ſenſation in the ſtomach from wind and other cauſes, often quickens (eſpecially in people whoſe nervous ſyſtem

---

* Act. Gotting. vol. ii. p. 140.

ſyſtem is very delicate and moveable) the motion of the heart; which will be often rendered ſlower again by a glaſs of generous wine, a dram of brandy, or any thing that, by invigorating the ſtomach, removes the uneaſy ſenſation in it.

A diſagreeable feeling in the ſtomach renders the heart more irritable, becauſe, by means of its nervous ſympathy with this organ, it increaſes its ſenſibility; and, in like manner, an inflammation or unuſual irritation in the kidneys or inteſtines increaſe the irritability of the ſtomach: but how a diſagreeable feeling in the ſtomach ſhould immediately alter the nature of the *gluten* of the fibres of the heart, in which *M. de Haller* places the irritability of this organ, is as inconceivable, as it is inexplicable upon any juſt principles of phyſics.

If therefore it appears, that the irritability of the moving organs of our body is increaſed as often as their own ſenſibility, or that of other parts with which they have a remarkable ſympathy, is increaſed; it will be thought, at leaſt, highly proba-

ble,

ble, that the irritability of any part depends upon its ſenſibility.

III. WHATEVER leſſens or deſtroys the ſenſibility of the muſcles of animals, alſo leſſens or deſtroys their irritability or power of motion.

THUS, when one's fingers or limbs have been long expoſed to ſevere cold, they not only become inſenſible, but paralytic. Frogs, bats, and other animals, with numbers of the inſect-tribe, are ſo benumbed by the winter's cold, as to be deprived of all feeling and motion: their blood does not circulate, nor their hearts beat; and their muſcles, tho' torn, cut in pieces, or otherwiſe ſtimulated, are not brought into contraction.

DURING the time of incubation, the chick's heart is obſerved to beat faſter or ſlower, and with more or leſs force, *i.e.* to become more or leſs irritable, as it is expoſed to greater or leſs degrees of heat; nay, after its motion has been ſtopt altogether by cold, a gentle heat will make it, in a very ſhort time, begin to contract anew [*].

FURTHER,

---

[*] Harvey De generat. animal. exercit. xvii.

Further, this *punctum ſaliens*, or heart of the chick, which, when touched with any thing capable of hurting it, is excited into quicker and ſtronger contractions, after being expoſed for ſome time to too great cold, is not affected by the moſt powerful *ſtimuli*.

It appears, therefore, that feeling and irritability are deſtroyed by cold, and reſtored by a proper degree of heat, and are ſo cloſely connected together, that the latter is never to be found where the former is totally wanting.

If authority could be of any weight in a matter which is to be determined by experiments and obſervations, we might ſupport our opinion with the name of one of the moſt judicious and ſucceſsful inquirers into nature that any age has produced. 'Ego pluribus experimentis certus ſum, '(*ſays the illuſtrious* Harvey), non motum 'ſolummodo puncto ſalienti ineſſe, ſed 'ſenſum etiam; nam, ad quemlibet, vel 'minimum, tactum, videbis punctum hoc 'varie commoveri, et quaſi irritari.---Vidi, 'inquam, ſæpiſſime, aliique qui una mecum "aderant,

"aderant, ab acus, ſtyli, aut digiti contactu, immo vero a calore aut frigore vehementiore admoto, aut cujuſlibet rei moleſtantis occurſu, punctum hoc varia ſenſus indicia, pulſuum nempe varias permutationes, ictuſque validiores ac frequentiores, edidiſſe; ut non dubitandum ſit, quin punctum hoc (animalis inſtar) vivat, moveatur, ac ſentiat." *De generat. animal. exerc.* xvii.

UPON occaſion of quoting Dr *Harvey*, it may not be improper to take notice of the error of thoſe who ſeem to think the irritability of the muſcles a late diſcovery *. If by *irritability* is meant that power by which muſcles contract when they are pricked, fretted, or otherwiſe ſtimulated, 'tis plain this was not unknown to *Harvey*; and many authors ſince his time might be named, who have particularly mentioned it †. But, if

---

* *Tiſſot. Diſcours preliminaire ſur l'irritabilité*, prefixed to his tranſlation of *M. de Haller*'s treatiſe of the ſenſible and irritable parts.

† The irritability of the heart, after being ſeparated from the body, was not unknown to *Galen* (*a*), but

(*a*) *Cor quamvis ex animali extractum fuerit, motionem diutius, tum in contractione, tum in dilatatione, ſervare conſpici. De Hippocrat. et Platon. decret.*

if by *irritability* be meant an active property of the muſcular glue analogous to gravity; this, it muſt be confeſſed, is a new diſcovery, though not likely to prove a laſting

it has been more particularly attended to by ſeveral phyſicians and philoſophers ſince the middle of the laſt century.

Swammerdam tells us, that, in diſſecting animals alive, he obſerved not only contractions in the muſcles, but in every muſcular fibre, though ſeparated from the body of the muſcle. Tract. de reſpiratione, cap. vii. § v. 1667.

Dr *Gliſſon*'s treatiſe *De anatomia hepat.* (1654) evidently ſhews, that he was well acquainted with the irritability of the muſcles; and, in his book *De ventriculo et inteſtinis*, (1677), he has ſeveral chapters on the irritability of the parts of the body; where he not only mentions the heart and inteſtines as endowed with this property, but tells us particularly that the fibres of the muſcles in dead animals are brought into contraction when acrid liquors are applied to them, cap. vii. No. 3. He gives ſeveral examples of the irritability of parts from ſympathy, and mentions the cauſes which may produce either too ſmall or too great a degree of irritability in the fibres, cap. ix. No. 4. 5. 6. and 7. He ſuppoſes irritability to ariſe from a natural perception in the fibres, without which they could be no more affected by any irritating cauſe than a deaf man is by ſounds. This natural perception he diſtinguiſhes from

ing one. *Opinionum commenta delet dies, naturæ judicia confirmat.*

But, to return from this digreſſion; *Opium*, which is remarkable for its power of

---

from feeling, concerning which he reaſons at great length, but with little perſpicuity, cap. vii.

Peyerus, after endeavouring to confute *Harvey*'s opinion of the chick's heart being not only endowed with motion, but alſo feeling, and aſcribing the irritability of this organ to its exquiſite but unknown ſtructure; adds, "Conſtat vero piſcium plurimos, nec non inſecta, et alia quædam animalcula, motus ſui aut vitæ admodum eſſe tenacia, adeo ut in partes quoque diſſecta ſeſe aliquamdiu adhuc motitent, imprimis ſi, adhibito ſtimulo, inſuper laceſſantur. Parerg. Anat. med. 7mum, pag. 200. Genev. 1681." The irritability of the inteſtines and heart was ſo well known to *Bohnius*, that he deduces the periſtaltic motion of the inteſtines from the irritation of the aliment, and aſcribes the alternate contraction of the heart *partly* to the *ſtimulus* of the blood ruſhing into its cavities, which had been mentioned before by *Harvey* and *Gliſſon* as the ſole cauſe of the heart's motion. Circul. anat. phyſiolog. p. 105. and 163. edit. 1686.

Baglivus has, in his book *De fibra motrice*, an intire chapter *De irritatione ſolidorum ſive ſtimulis, et variis ſtimulorum effectibus*: from which it appears, that he was far from being ignorant of the power of *ſtimuli* to excite the parts of living animals into contraction. He has alſo ſeveral experiments concerning the irritability

of

of impairing or deſtroying the ſenſibility of all the parts of the body, alſo leſſens or ſuſpends the irritability or moving power of the muſcles. Thus, in a ſmall doſe, it

---

of the heart after being ſeparated from the body, and mentions particularly that frogs are convulſed by punctures an hour after they have been deprived of all the *viſcera* of the *thorax* and *abdomen*. Exper. xi. *de circulatione ſanguinis in rana.*

Among the later writers, Dr *De Gorter* has, in many places of his works, taken notice of the motions of ſuch parts of animals as are irritated; and obſerves, that theſe motions are not to be accounted for from elaſticity. "Sed præterea, cum omnes fibræ nervoſæ "vellicatæ ſeſe inordinate et involuntarie moveant, "patet minimam cauſam ſæpe ſufficere ad totam cor"poris œconomiam turbandam.—Cur autem a velli"catione pars aliqua nervoſa ſtatim contrahitur, diffi"cile explicatur; veritas autem ejus aſſerti ubique ma"nifeſta eſt, non modo in nervo iſto vellicato, ſed et "in reliquis ſurculis nerveis ab eadem origine venienti"bus, ut in ſternutatione, tuſſi, vomitu, &c. Sentio "id eſſe adſcribendum Summi Opificis placito, qui vo"luit corpus noſtrum ita concinnare, ut ſtatim ac vel"licetur pars nervoſa, ibidem demandentur ſpiritus; "hoc enim ab elaſticitate partium derivare, vellica"tione vel ſtimulo agitatarum et oſcillantium, fruſtra "tentarunt multi." *Gorteri Compend. medicinæ*, vol. 1. p. 58. & 63. Lugd. Batav. 1735.

Dr

puts a ſtop to vomiting and coughing; and quiets the convulſive motions of the *inteſtinum rectum*, bladder, abdominal muſcles, and diaphragm, in a *teneſmus* and ſtrangury, although

Dr *Monro*, in his Anatomy of the nerves, tells us, "That all muſcles, but eſpecially the heart, continue "to contract, in an irregular way, for ſome time after "they are cut away from the animal to whom they "belonged; and that, after this motion of theirs has "ceaſed, it may be reſtored again by breathing upon "them, or pricking them with a ſharp inſtrument." Anatomy of the human bones and nerves, p. 38. edit. 3. 1741.

M. de Haller, ſpeaking, a dozen of years ago, of the motion of the heart in time of ſleep, ſays, "Cæterum "tota theoria iſta ſimpliciſſimo phænomeno, a nemine "negabili, nititur, omnem fibram muſculoſam animalis vivi, irritatam a quacunque cauſa, continuo in "contractionem ire, ita ut hæc ipſa ultima nota ſit, qua "animalia imperfecta a vegetabilibus dignoſcantur." And afterwards, with regard to the motion of the heart after its ſeparation from the body, he expreſſes himſelf thus; "Omnino videtur quod alibi faſſus ſum, "cum PRÆCEPTORE, in fibra animali aliquam ad irritationes contractilitatem ſupereſſe, quæ ſimplici elatere fortior, a motu muſculari diverſa, quod cerebri "cordiſque non indiga ſit, et in ipſa hujus fibræ humidæ adhuc et integræ fabrica ſundata eſſe videtur." Boerhaav. Prælect. academ. vol. iv. p. 586. & 616. 1743.

Dr

although the *ſtimuli*, which produced theſe motions, continue to act on the parts: when given in much larger quantity, it ſuſpends the periſtaltic motion of the inteſtines, and

Dr *Winter*, in 1746, publiſhed an oration *De certitudine in medicina practica*, wherein, it is ſaid, he has referred all the motions of the human body to the irritable nature of the fibres and the power of a *ſtimulus*; acknowledging, however, with *Baglivi*, the *dura mater* as the fountain from which all our motions ſpring. But this piece I have not yet had the good fortune to ſee.

In an Eſſay on the vital and other involuntary motions of animals publiſhed in 1751, the author, after conſidering particularly three kinds of contraction obſervable in the muſcles of animals, *viz.* natural, voluntary, and involuntary from a *ſtimulus*, endeavours to ſhew, that all the vital and involuntary motions are owing to *ſtimuli* irritating either the organs moved, or ſome part with which they have a particular ſympathy; that the alternate contractions, excited in muſcles by irritating ſubſtances applied to them, proceed from their ſenſibility, and are no more than an effort of nature to throw off what is hurtful: from which he concludes, that, if the ſenſibility of the muſcles be not a property of the matter of which they are compoſed, but owing to a ſuperior principle animating them, all the vital and other involuntary motions muſt ultimately be aſcribed to the active power of that principle.

makes the heart contract more ſlowly, till being by degrees rendered quite inſenſible, its motion ceaſes altogether.

BUT,

Laſtly, *M. de Haller*, in his treatiſe *De partibus corporis humani ſenſibilibus et irritabilibus*, publiſhed in vol. 2. of the *Gottingen* Tranſactions in 1753, has, by a great many curious experiments, proved, not only that all muſcular fibres, and them alone, are endowed with irritability or a power of alternate contraction, but has alſo ſhewn, that ſome muſcles and organs are poſſeſſed of this power in a greater degree than others. He has further endeavoured to prove, that the irritability of the muſcles is independent of the nervous power, and has no connexion with ſenſibility, but is owing to the glutinous matter of the muſcular fibres.

From what has been ſaid, together with the ſhort hiſtory of irritability given by *M. de Haller*, (Act. Gotting. vol. ii. p. 154. &c.), it appears, that the contractile power of ſtimulated muſcles has been long known to phyſicians, tho' within theſe few years paſt, it has been made the ſubject of more particular inquiry.

⁂ After the mention made here of *M. de Haller*'s hiſtory of irritability, and of the reſult of his own experiments on this ſubject, I did not expect that he would have complained of my want of equity, in not taking notice of what he had ſaid on thoſe heads, or have alledged, that I had purpoſely omitted doing him this piece of juſtice, leſt it ſhould have weakened a cenſure which I intended againſt him, as having aſſumed

BUT, as *M. de Haller*, who allows that *opium* deſtroys the irritability of the ſtomach, inteſtines, and other muſcles, denies it to have any power over the heart, * and ſeems to call in queſtion thoſe experiments of mine which ſhew, that *opium*, injected into the ſtomach and guts of frogs, renders the motion of the heart much ſlower than uſual, and at laſt puts a final ſtop to it †; I thought it neceſſary to endeavour to clear up this matter by ſome farther experiments, which I ſhall here briefly relate.

(*a*) JUNE 5. 1755, at 18 minutes paſt four in the afternoon, I injected a turbid ſolution of half an ounce of *opium* in eight ounces of water, into the ſtomach and guts of a frog; and, as it ſquirted out moſt of the ſolution injected by the *anus*, I threw in ſome more in its place. At 24 minutes paſt five the ſame evening, I opened this frog, and obſerved the heart beating very ſlowly, not above ſeven times in a minute; when

---

ſumed to himſelf the diſcovery of the irritable power of muſcular fibres. See *Memoires ſur les parties ſenſibles*, &c. vol. iv. p. 124. and 125.

* Act. Gotting. vol. II. p. 147. 154. and 157.

† Eſſay on vital motions, p. 370, *&c.*

when it was touched with the point of a pair of ſciſſars, it renewed its motion faſter for two or three pulſations; after which it became as ſlow as before. The other muſcles of this frog were not at this time brought into contraction by pricking or tearing their fibres.

(β) I laid open the whole *abdomen* and *thorax* of a frog; and, at 28 minutes paſt ſeven in the morning, immerſed it in a turbid ſolution of *opium, viz.* the ſame that was made uſe of in the preceeding and following experiments. At forty minutes after ſeven, I turned the frog on its back, and obſerved its heart beating between ten and eleven times in a minute. Having laid it again on its belly, that it might be more expoſed to the action of the *opium;* at forty eight minutes paſt ſeven, I turned it again on its back, and obſerving the heart without motion, I opened the *pericardium;* which producing no effect, I cut the heart out of the body, and laid it on a plate, when it contracted twice or thrice, and never after moved, although it was pricked once and again with a pin.

(γ) I CUT off a frog's head, and intirely deſtroyed its ſpinal marrow by puſhing a ſmall probe down through the ſpine, which occaſioned ſtrong convulſions of all the muſcles, eſpecially thoſe of the inferior extremities. Ten minutes after this, I opened the *thorax*, and found the heart beating 45 times in a minute. Sixteen minutes after decollation and the deſtruction of the ſpinal marrow, it moved 40 times in the minute. After half an hour, it made 36, and, after fifty minutes, only 30 pulſations in the minute, which were now become very ſmall and feeble.

N. B. WHEN the *thorax* of another frog was opened immediately after decollation and the deſtruction of its ſpinal marrow, its heart beat 60 times in a minute.

(δ) I CUT out the heart of a frog, and put it into fountain-water, at twenty three minutes paſt twelve. After twelve minutes immerſion, I took it out of the water, when it beat 20 times in a minute. Having immerſed it for five minutes more, it ceaſed from motion; and when taken out of the water, did not move except when pricked, and then only performed one pulſation.

EIGHT

(*e*) EIGHT minutes paſt eleven, I cut out the heart of another frog, and immerſed it in fountain-water, 28 minutes after eleven, it continued to move: but its motion, tho' at the rate of eleven pulſations in thirty ſeconds, was confined to about one third of the heart next its *apex*. Two minutes after this, obſerving it without any motion, I took it out of the water, and laid it on a table, where it remained at reſt, unleſs when touched. Soon after this, however, it began to move, and, at 25 minutes after immerſion, performed 9 pulſations in 63 ſeconds.

(*f*) I cut out the heart of a frog, and, at 32 minutes paſt ten, immerſed it in a turbid ſolution of *opium* in water of the ſame degree of heat with the fountain-water uſed in the two laſt experiments *. After this heart had been immerſed ten minutes I took it out of the ſolution, and laid it on a table; but it made not the ſmalleſt motion: and, when pricked with the point of a knife, though it quickly recovered its ſhape, yet it was not excited into a proper contraction as the heart of . I continued to obſerve

---

* *Viz.* nearly 60 degrees of *Farenheit*'s thermometer.

obſerve this heart from time to time for above half an hour, but it never made the leaſt motion.

(η) I CUT out the heart of another frog, and put it into the ſame turbid ſolution of *opium;* after ſeven minutes immerſion, I took it out, and laid it on a plate, where it remained at reſt. When pricked with a knife, it did not perform a full pulſation, but ſeemed to feel a little, by a very faint kind of motion which was excited in ſome of its fibres.

(θ) MR ROBERT RAMSAY ſtudent of phyſic *, at my deſire, made the following experiment. After making an opening into the cavity of the *abdomen* of a ſmall dog near ſix months old, he injected by the wound a dram of *opium* diſſolved in two ounces and a half of water; but, before he could ſtitch up the wound, about an ounce of the ſolution eſcaped. Four minutes after making the injection, he laid bare the *thorax*, by diſſecting off the teguments which did not ſeem to give the dog any pain; and could plainly feel the motion of his

---

* Now Fellow of the Royal College of Phyſicians in *Edinburgh*.

his heart through the *pleura.* It beat 76 times in a minute, but became gradually ſlower *. Immediately after counting the pulſe, Mr *Ramſay* cut the ribs on each ſide of the *ſternum*, which he laid back in the uſual way. The heart, which was thus brought into view, appeared quite turgid, and continued in motion about five minutes: during which time it performed only between 60 and 65 weak vibrations; for they were not compleat contractions. While the heart was thus moving, warm ſpittle was firſt applied to it, then cold water, and, laſt of all, oil of vitriol, which ſhrivelled the parts it touched almoſt in the ſame manner as a hot iron would have done; but none of them accelerated the heart's vibrations, which became gradually ſlower, till they ceaſed altogether.

AGREEABLY to this experiment, we are told by Dr *Alſton*, in his learned Diſſertation on *opium*, that a filtrated ſolution of this medicine in water having been injected into the veins of a dog, his pulſe, which, when he was firſt ſeized with convulſions, was

---

* This dog's heart, before the injection of the *opium*, beat 150 in a minute.

was rendered quick and ſmall, became afterwards full and ſlow [*]. And Dr *Kaau Boerhaave* informs us, that in a ſmall dog, which he opened ten hours after he had ſwallowed three grains of *opium*, the motion of the heart and arteries was very ſlow [†].

FROM theſe experiments it evidently appears, that, as *opium* deſtroys the ſenſibility of all the parts of the body, ſo it deprives the muſcles of all power of motion; nor does the heart in this reſpect poſſeſs any privilege above the other muſcles, except that its moving power is not ſo ſoon deſtroyed by *opium* as theirs.

HOW *M. de Haller* came to be ſo greatly deceived as to this matter, I cannot pretend to conjecture; ſince he has not told us in what manner his experiments were made: but, it is not to be doubted, that his candor and love of truth will make him readily acknowledge his miſtake, as ſoon as he ſhall diſcover it.

---

[*] Vid. Medical Eſſays, vol. 5. p. 1. art. xii.

[†] Cor lentiſſime movebatur. Motus in arteriis (ſcil. duræ et piæ matris) debilis et valde lentus. Vid. Impet. faciens *Hippocrati* dictum. No. 434. et 436.

IV. When a viper is pricked with the point of a knife three days after being deprived of its head, heart, and other *viſcera*, it moves, not only the muſcles whoſe fibres are touched, but alſo the other muſcles of its body which have no connexion with thoſe that are ſtimulated. This indicates either a ſympathy between theſe muſcles, which ſuppoſes feeling, or ſome general active principle animating them, which, being affected with a diſagreeable ſenſation by the *ſtimulus* applied to any one muſcle, brings many others into action, in order to avoid what is hurtful to it. In like manner, when a few drops of boiling water fall on one's leg, the muſcles which ſerve to move this member are inſtantly and involuntarily brought into contraction, in order to remove it from the offending cauſe.

A frog, after it has been deprived of its head, will, when touched, often jump and move about for a very conſiderable time; and it is obſervable, that, when the toes of its hind feet are any way ſtimulated, it conſtantly draws them up to its body; nay,

nay, if, when they are in this ſituation, the toes are again irritated, the legs and feet are not extended, but brought ſtill cloſer to the body. If one of the legs is pulled down from the body and kept in an extended ſtate, no ſooner are the toes of this foot wounded than the leg is drawn up to the body as before. Now, if theſe motions were owing to ſome property of the inſenſible matter of which the muſcles are compoſed, Why ſhould not an irritation of the toes be ſometimes followed by a contraction of the extenſor as well as the flexor muſcles of the legs and thighs? But, if we allow them to be owing to the painful ſenſation in the toes, we ſhall ſee that the frog does, in this caſe, with its limbs, juſt what a ſnail does with its horns, when they are roughly touched.

AGAIN, it is very remarkable, that, when the toes of a frog are pricked or otherwiſe wounded inſtantly after decollation, there is either no motion produced in the muſcles of the legs at all, or a very inconſiderable one. But, if the toes of a frog be touched with one's finger ten, fif-

teen, or twenty minutes after decollation, the legs and thighs are immediately drawn up to its body; and, if they be at this time wounded, pricked, or cut with a penknife, the muſcles, not only of the legs and thighs, but alſo of the trunk of the body, are, for the moſt part, ſtrongly contracted, and the animal ſometimes moves from one place to another.

Is not the irritation of the toes, immediately after decollation, rendered ineffectual to produce any motion in the muſcles of the legs and thighs, by the greater pain occaſioned by cutting off the head *? And are not the muſcles of the poſterior extremities, as well as of the trunk of the body, brought into action by wounding the toes fifteen or twenty minutes after decollation, becauſe the pain produced by cutting off the head is now ſo much leſſened (perhaps wholly obliterated) as not to prevent the animal from feeling very ſenſibly when its toes are hurt?

It were to be wiſhed that thoſe who chuſe

---

* Duobus doloribus ſimul obortis, non in eodem loco, vehementior obſcurat alterum. Hippocrat. Aphor. lib. 2. No. 46.

chuſe to account for the irritability of the muſcles, not from their ſenſibility, but from ſome unknown property of the matter compoſing them, would, inſtead of moving objections concerning the ſeat of the ſoul, its extenſion, diviſibility, and manner of co-exiſting with the body, favour us, if they can, with ſome probable explication of the *phænomena* above mentioned.

V. That the motions of irritated muſcles are owing to the ſenſation excited by the *ſtimulus* applied to them, will appear highly probable, if we conſider, that we are, in fact, conſcious of many involuntary motions in our bodies proceeding from a particular ſenſation, either in the organs moved, or in ſome neighbouring part. This is the caſe with the motions of the ſtomach and diaphragm, in vomiting and the hiccup, of the great guts and diaphragm in a *teneſmus*, of the *acceleratores urinæ* in expelling the *ſemen*, and of the intercoſtal muſcles and diaphragm in ſneezing, coughing, and ſometimes even in breathing; nay, when, by ſudden fear or any great ſurpriſe, the heart is ſet a palpitating,

tating, we have a particular feeling in this muſcle, partly from the blood ruſhing ſuddenly and in too great quantity into it. More examples might be given; but theſe may ſuffice to ſhew the connexion there is betwixt the ſenſibility and irritability of the moving organs of our body.

Upon ſuppoſition that the motions of irritated muſcles did not proceed from any kind of feeling, but from ſome inanimate cauſe, their contractions ſhould be all, either regularly alternate, or equable and uninterrupted, like the falling of the leaves of the ſenſitive plant *; but we find, that, while moſt of our muſcles are brought, by the action of *ſtimuli*, into alternate contractions, there are ſome few which contract uniformly and equably during the time the *ſtimulus* operates, without any intermiſſions or alternate relaxations. Of this kind is the contraction of the diaphragm and abdominal muſcles when the *inteſtinum*

---

* I have elſewhere ſhewn by experiments, that the falling of the leaves of the ſenſitive plant, when touched, does not indicate any kind of feeling, and is no way ſimilar to the alternate contractions of irritated muſcles. Eſſay on vital motions, *&c.* p. 245.

inteſtinum rectum is irritated, of the *ſphincter pupillæ*, while the ſame degree of light continues to act on the *retina*, and of the muſcles of the internal ear as long as the ſame ſound is applied to this organ. Nay, the diaphragm, which is brought into one continued contraction by a *ſtimulus* affecting the *inteſtinum rectum*, is agitated with alternate convulſions from an irritation of the left orifice of the ſtomach, or the olfactory nerves. What account can poſſibly be given of this, upon ſuppoſition that theſe motions proceed from the *gluten* of the muſcular fibres? or what difference can it make to this inſenſible *glue*, whether the *ſtimulus* be applied to the noſe or *anus*? But, allowing theſe motions to ariſe in conſequence of an uneaſy ſenſation in the part ſtimulated, it will immediately appear, that they are performed in ſuch manner as is moſt effectual to leſſen or remove the irritating cauſe *.

Again, if the motions of muſcles from *ſtimuli* were not owing to a feeling, How could the convulſive motions of the diaphragm

---

* Vid. An Eſſay on the vital and involuntary motions, p. 258. *&c.*

phragm in the hiccup be often immediately ſtopt by ſudden fear, joy, or grief? Why ſhould an irritation of the olfactory nerves become ineffectual to produce ſneezing, when ſome of the muſcles of the back or *thorax* are affected with a rheumatiſm? And why ſhould the convulſive motions of the ſtomach and diaphragm in vomiting, be frequently interrupted by extraordinary fear, or any very great and ſudden ſurpriſe? It will be difficult, nay impoſſible, to give any ſatisfactory ſolution of theſe *phænomena*, if the motions of irritated muſcles are ſuppoſed to proceed from ſome unknown property of their *inſenſible glue*: but they are at once intelligible and clear, upon ſuppoſition that they are owing to an uneaſy ſenſation; for as often as this feeling is overpowered by a ſtronger one in ſome other part of the body, or when the mind is ſo ſuddenly and ſtrongly affected by external objects, as, for a ſhort time, to become almoſt inſenſible of the irritation, the motions owing to it muſt be leſſened or ceaſe.

GRAVITY, magnetiſm, and electricity, are all

all regular and uniform in their operations, and beſpeak nothing of feeling or life in the bodies which are endowed with them, and may therefore be ſuppoſed to proceed immediately from material cauſes; altho' the activity of theſe cauſes muſt be, at laſt, referred to the great ORIGIN of all power and life in the univerſe. But the motions of animal bodies from a *ſtimulus* are, in many caſes, ſo plainly perceived to flow from an uneaſy feeling, their various *phænomenà* can be ſo eaſily explained upon this ſuppoſition, and are ſo unaccountable on any other, that it is matter of no ſmall wonder to find many learned and ingenious phyſiologiſts uſing their utmoſt efforts to overthrow this opinion, and ſtruggling, but in vain, to derive thoſe motions from inanimate matter.

LIFE, ſenſe, and proper activity, ſeem to be inconſiſtent with the known properties of matter; wherefore, when we ſee a ſyſtem of matter endowed with theſe, we may, without preſumption, conclude, that they are owing, not to the material ſyſtem alone, but to ſome active principle animating

ting it. And altho', even upon this ſuppoſition, it may be very difficult to account for ſome of the motions obſerved in ſuch a ſyſtem, or in its parts when ſeparated, we cannot hence conclude, that they are not owing to any ſuch power; but only that our ignorance of the nature of immaterial beings, and of their particular union with, and manner of acting upon bodies, throws a veil of obſcurity over theſe things, which the moſt enlightened philoſopher will never be able to remove.

M. DE HALLER, towards the end of his performance, has thrown out ſome reflections upon my manner of writing, and the few experiments I had made on dying animals; which, tho' it were eaſy to obviate, I ſhall paſs by unnoticed, from a conſciouſneſs of their being ill-founded, a diſlike of introducing any thing perſonal into a philoſophical debate, and a perſuaſion that my learned adverſary himſelf will not, upon a *cool* review, intirely approve of them.

APPENDIX,

CONTAINING A

REVIEW of the CONTROVERSY

CONCERNING THE

Senſibility and Moving Power of the Parts of Men and other Animals;

IN ANSWER TO

M. de HALLER'*s late Remarks on theſe Subjects in the 4th Volume of the* Memoires ſur les parties ſenſibles et irritables.

Reſellere ſine pertinacia, et reſelli ſine iracundia, parati ſumus.
CICERO, Diſput. Tuſcul. lib. 2.

A

REVIEW *of the* CONTROVERSY

CONCERNING THE

Senſibility and Moving Power of the Parts of Men and other Animals.

HAVING, in the notes to the preceeding obſervations, obviated many of *M. de Haller*'s remarks of leſſer moment, I ſhall now proceed to give a full anſwer to thoſe which relate to the chief points in debate between us; and this I ſhall endeavour to do in ſuch a manner, as to ſhew that it is not prejudice, but facts, which influence my judgment *.

BUT, before I enter upon my ſubject, I muſt obſerve, that although my learned adverſary has been at great pains, in ſeveral places of his writings †, to repreſent

---

* *Memoires ſur les parties ſenſibles et irritables*, tom. iv. p. 62.

† *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 61. 62. and 101.

me as a diſciple of *Stahl*, and quite pre-occupied with his opinions; yet the reader will be apt to queſtion the juſtice of this accuſation, after being told, that one of the lateſt and ableſt defenders of the Stahlian doctrine *, has not only attacked my theory of the vital motions, but thinks it leſs probable than even that of *M. de Haller* and other mechanical phyſicians. The truth is, I have endeavoured to ſhew the opinion of *Stahl* and his followers, with regard to the ſoul's governing and directing the vital motions, to be altogether improbable †. I have derived all our involuntary motions, whether vital or not, from the irritation of the different organs by various *ſtimuli.*---I have ſhewn that irritation only excites motions in our muſcles, by affecting them with a diſagreeable ſenſation; and therefore, unleſs ſenſibility may be a property of matter, that the vital and other involuntary motions muſt, at laſt, be referred to the mind, or ſome ſentient principle animating the body ‡.

PART

---

* Dr *Porterfield*, in his Treatiſe on the eye, vol. 2.

† Eſſay on the vital motions of animals, Sect. xi.

‡ Eſſay on the vital motions, Sex. x. and xi.

P A R T I.

Of Senſibility.

AS I have allowed the tendons, ligaments, *dura mater*, and moſt other membranes, to be poſſeſſed of no more than a very inconſiderable degree of ſenſibility in a ſound ſtate, and have already ſaid enough to ſhew that the *cornea* and marrow are not inſenſible, and that the kidneys are much leſs ſo than *M. de Haller* has alledged; the only points, of any conſequence, with regard to the ſenſibility of the parts of animals, which remain to be diſcuſſed, are, 1. Whether thoſe parts, which have little or no feeling in a ſound ſtate, may not ſometimes acquire a conſiderable degree of ſenſibility when they are diſeaſed. 2. Whether, from the alledged total inſenſibility of the tendons, ligaments, and membranes, or the inviſibility of their nerves, we can reaſonably conclude, that they have really no nervous filaments beſtowed upon them.

I. WITH regard to the firſt of theſe, I

had obſerved, that thoſe parts which are by all allowed to be ſenſible, acquire a more acute feeling when they are inflamed; that the parotids, tonſils, lymphatic glands, thoſe of the *mammæ*, and others, which have no acute feeling in a natural ſtate, and are often much ſwelled without giving any uneaſineſs, become very ſenſible when they are inflamed, and occaſion ſevere ſhooting pains. From analogy, therefore, one would be apt to conclude, that the tendons, ligaments, and membranes may, in a diſeaſed ſtate, acquire ſometimes a conſiderable degree of feeling. But as a direct proof of this, I have ſhewn that in wounds and ulcers, the *tela cellularis* becomes often very ſenſible; that in morbid caſes the *dura mater*, cartilages, ligaments, and membranes, when laid bare, often granulate, and are ſenſible of every acrid ſubſtance applied to them; that tho' the bones be inſenſible in a ſound ſtate, yet after fractures, or when they exfoliate, there ariſes from them a ſoft fleſhy ſubſtance, which is at firſt remarkably ſenſible, but gradually loſes its feeling as it grows harder, till,

at laſt, it becomes an inſenſible *callus* or bone *.

I have ſhewn that the tendon of the *biceps* muſcle of the arm, after being wounded in blood-letting, has not only been inflamed, but found ſwelled to ten times its natural ſize; and that thoſe ſymptoms, which have been aſcribed to a wound of the tendon of this muſcle, are never obſerved after bleeding at the cephalic or jugular veins, although in the laſt caſe ſome ſmall nerves are frequently wounded †.

THE ſwelling and rigidity brought on the joints, and the calcarious matter collected within their *capſulæ*, by frequent attacks of the gout, ſhow, that this diſeaſe has

---

* In the preſent diſpute, the concluſion is the ſame; altho' the granulation in fractures and exfoliations be ſuppoſed with *M. du Hamel* to ariſe from the *perioſteum*; ſince *M. de Haller* allows as little ſenſibility to this membrane as to the bones themſelves.

† *M. Muhlmann*, tho' he has endeavoured to confirm *M. de Haller*'s doctrine of the inſenſibility of the tendons and *perioſteum*, yet he acknowledges, that in certain caſes the tendons are affected with violent pain. "J'ai remarqué les plus fortes, et les plus in-"ſupportables douleurs dans les tendons entrés en "putrefaction ou ſuppurés." Vid. *Memoires ſur les parties ſenſibles*, tom. ii. p. 143.

has its ſeat, not in the ſubcutaneous nerves, but in the tendons and ligaments of the articulations. The pain and ſwelling in conſequence of a ſprain of the wriſt or ancle cannot reaſonably be aſcribed to the over-ſtretching of the ſubcutaneous nerves: the weakneſs of the joint for many weeks or even months, and the pain occaſioned by moving it, clearly ſhew the ligaments and tendons to be the parts which had ſuffered. The great increaſe of pain occaſioned by the leaſt motion of joints affected with the rheumatiſm, proves that this diſeaſe has alſo its ſeat in the tendons and ligaments, which laſt can in ſome caſes be perceived to be conſiderably ſwelled. Laſtly, I have obſerved, that the *dura* and *pia mater* have been found inflamed and mortified in patients, who have died of a *phrenitis*, and that the headach in the beginning of fevers, has not commonly its ſeat in the teguments of the ſkull, but in the *dura* or perhaps the *pia mater*.

In anſwer to theſe, and ſeveral other arguments to prove that thoſe parts, which in a ſound ſtate have very little feeling, may

may be rendered very ſenſible by diſeaſes, *M. de Haller* is pleaſed to ſay, That my obſervations are vague and without preciſion *: That to a tendon found inſenſible, I ought to oppoſe a tendon which felt the ſtroke of the lancet, and to a *dura mater* burnt without pain, an inſtance of convulſions occaſioned by burning that membrane. But is not this reaſoning plainly evaſive? For, ſince I do not alledge that the tendons or *dura mater*, in a ſound ſtate, are poſſeſſed of any other than a very obtuſe ſenſation, all that is incumbent on me, is, to prove that thoſe and other parts reckoned inſenſible by *M. de Haller*, are ſometimes ſo changed by diſeaſes, as to be endowed with a painful feeling.

M. DE HALLER has cited from *M. Vari* an inſtance of a patient, in whom the *pleura* was found mortified after death, although he had never had any more than an obtuſe pain in his *thorax* †. Inſtead of criticiſing on this caſe, which is not related with ſufficient

---

* *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 105. 106.

† *Vid. Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 26. & tom. II p. 427.

ficient preciſion to draw any certain concluſion from it*, I ſhall oppoſe to it the following proof of the ſenſibility of the *pleura* in a man of 21 years, upon whom Dr *Middleton*, phyſician at New-York, performed the operation of the *empyema*. The Doctor, in the account of this patient's caſe, which he was pleaſed to tranſmit to me, has the following paragraph. "Having diſſected down to the *pleura*, the muſcular fibres were thruſt aſide with the "handle of the ſcalpel, 'till it was expoſed, "for about one half inch in length; it ſeemed of a pale whitiſh colour, and ſounded "ſomewhat like a drum; the edge of the "knife being now moved very cautiouſly "and lightly along the expoſed *pleura*, we "obſerved that the patient changed the "ſtile of his complaining, and cried out "with ſome vehemence that the pain now "went to his heart. I immediately recollected the diſpute between Dr *Haller* and "Dr

---

* Phyſicians are not ignorant, that according to the degree of the diſeaſe, and the conſtitution of the patient, inflammations are attended with very different degrees of pain; and that, after death, ſmall mortifications are ſometimes obſerved, even in the ſtomach and inteſtines, altho' no very violent pain has preceeded.

" Dr *Whytt* relating to the ſenſibility or in-
" ſenſibility of membranes, and, before the
" *pleura* was yet penetrated, I aſked him if
" the cutting now gave him more pain
" than before? he anſwered very readily
" in his way, Oh yes, it ſome way goes to
" my very heart."

THE *pleura* ſeems to have been much more ſenſible in this patient, than it is ever in a natural ſtate, and altho' it did not appear red, yet from the collection of *pus* in the *thorax*, it could not fail to be diſeaſed, and probably, in ſome degree, inflamed.

I have formerly mentioned the bad conſequences obſerved by my worthy colleague Dr *Monro ſenior* after piercing the capſular ligament of the knee-joint, to let out a watery fluid which had been collected there; and have now to add, that the induſtrious Dr *Reimarus* has lately given a particular account of three patients, in whom the piercing or wounding of this ligament was followed, after ſome days, with violent pain, ſwelling, and fever *. My learned friend Dr *Simſon* long before, not only obſerved

---

* Diſſert. de tumore ligamentorum circa articulos, p. 15. et 16.

ſerved the ſame ſymptoms in a patient whoſe caſe he deſcribes, but tells us that the moſt acute pain was felt, when the diſtended *capſula* of the knee-joint was cut *. Nay, even the caſe mentioned by Mr *Warner* is a proof, that the ligaments of the joints are not inſenſible in a morbid ſtate; for tho' the patient had a perfect recovery in about 12 weeks, yet he was much pained, not only for ſeveral hours after the operation, but alſo at other times †.

But to theſe, and indeed every other inſtance that can be produced, of pain occaſioned by cutting thoſe parts which *M. de Haller* reckons inſenſible, he has found a ready anſwer, by affirming, that in ſuch caſes, the pain is not owing to thoſe parts themſelves, but to ſome ſmall nerves paſſing along their ſurface ‡. And particularly he aſcribes the ſenſibility of the cellular membrane below the ſkin, in an inflamed ſtate, to ſome ſmall nervous branches

---

* Edinburgh Medical Eſſays, vol. iv. art. 20.

† Vid. Philoſophical tranſactions, vol. xlix. p. 457. compared with *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 59.

‡ *Memoires ſur les parties ſenſibles*, tom. iv. p. 105. et 106.

es paſſing through it, or to the interior *lamellæ* of the true ſkin, which have been miſtaken for the *tela celluloſa*. But this laſt obſervation can no ways account for the pain in thoſe ulcers that are deeper than the ſkin; and if the firſt were true, every part of the inflamed cellular membrane ought to be inſenſible, except thoſe places where the branches of the nerves traverſe it; whereas every point of a ſuppurated wound in the cellular ſubſtance is acutely ſenſible, as ſurgeons daily obſerve. But further, whence comes the ſenſibility of that ſoft granulated ſubſtance, which ſometimes ariſes from the ſurface of the bones, cartilages, and membranes? It cannot be ſo much as pretended, that there are any branches of nerves running along the ſurface of this newly generated ſubſtance.

ALTHO' undoubtedly the pain occaſioned by wounding thoſe parts which are reckoned inſenſible by *M. de Haller*, may ſometimes be owing partly or chiefly to ſome ſmall nervous filaments running along their ſurface, but not belonging to them; yet to aſſert that this is always the

caſe,

caſe, ſeems to be a mere ſubterfuge, and is a ſtrong indication of the weakneſs of that cauſe for whoſe ſupport ſuch an argument is neceſſary. If the pain which Dr *Middleton*'s patient felt, when he touched the *pleura* gently with the point of his knife, was owing to ſome nerve running along its ſurface, why was not as great a pain felt in cutting the ſkin, *tela celluloſa*, and intercoſtal muſcles, which could not well be done without wounding ſeveral ſmall nerves? The Doctor's particular attention in making this experiment, renders it probable that any conſiderable nerves running along the ſurface of the *pleura* would not have eſcaped his notice; and if there had been but one branch, it was ſcarce to be expected, he would have hit upon it, not only when he firſt gently touched the *pleura*, but alſo afterwards when he cut it. Thoſe patients mentioned by Dr *Reimarus*, who ſuffered ſo much after piercing the capſular ligament of the knee-joint, felt but little when the wound was made, nor did the violent pain come on for ſeveral days *. This, however, could

---

* Diſſert. de tumore ligament. p. 15. et 16.

could not have been the caſe, if their bad ſymptoms had been owing to the cutting or wounding ſome ſmall branches of nerves. Further, the pain was felt thro' the whole ligament of the joint, and not chiefly or ſolely in the wounded part; which, however, muſt have happened, had it proceeded from the nerves *.

I ſhall only add, that ſince it appears from certain obſervations, that the membranes, tendons, and ligaments are liable to inflammation, it will likewiſe follow that they are in ſuch a ſituation ſenſible. Phyſicians and ſurgeons know that the ſeveral parts of the body acquire a more acute feeling by inflammation; that the more ſenſible any part is, the more apt it is to be inflamed by irritation, and that ſuch *calli* as are quite inſenſible are alſo incapable of inflammation. Indeed, to aſſert that any part may be inflamed, and yet remain altogether inſenſible, is a paradox that will hardly go down with any one who has been much converſant in practice.

But it will ſtill be objected to what has

---

* Diſſert. de tumor. ligament. p. 17.

been ſaid, How can we ſuppoſe thoſe parts to be endowed, in any ſtate, with any degree of feeling which are deſtitute of nerves, and which numberleſs experiments on living animals have ſhewn to be inſenſible *? In anſwer to which, it is proper to obſerve, that *M. de Haller*'s experiments do not demonſtrate either the tendons, ligaments, or membranes to be *altogether* inſenſible, any more than the liver, ſpleen, kidneys, and ureters; for when theſe laſt parts were pricked or cut, the animals ſhewed no ſigns of pain †. If therefore we know, from undoubted obſervations on men, that the ureters, kidneys, *&c.* are not deſtitute of feeling, it will follow that the tendons, ligaments, and other parts reckoned wholly inſenſible by *M. de Haller*, may, notwithſtanding his experiments, be alſo endowed with ſome ſmall degree of feeling, and conſequently may, like other parts of the body, acquire a greater ſenſibility when they come to be in an inflamed ſtate.

If,

---

* *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 106.

† Act. Gotting. tom. ii. p. 131.; et *Memoires ſur les parties ſenſibles*, tom. iv. p. 33.

If, indeed, it could be proved that the tendons, ligaments, and membranes are really deſtitute of nerves, *M. de Haller's* concluſion would certainly follow, *viz.* that in no caſe could they become the ſeat of painful diſeaſes. But this leads me to the ſecond point which I propoſed to conſider, *viz.* Whether, from the alledged inſenſibility of any part, or the inviſibility of its nerves, we can juſtly conclude that it has none.

II. M. de Haller is of opinion that the tendons, ligaments, cartilages, and membranes, have no nerves. Firſt, Becauſe if they were furniſhed with nerves they could not be inſenſible; and 2*dly*, Becauſe anatomiſts have not been able to trace their nerves, either with the diſſecting knife, or to diſcover them by the help of the microſcope *.

(1.) With regard to the firſt of theſe arguments, we have already ſhewn that *M. de Haller's* experiments only prove the parts in queſtion to be deſtitute of any painful feeling, but not that they are altogether

---

* *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 28. &c.

together inſenſible; and it is moſt certain, that a part may be furniſhed with nerves, and yet have little or no ſenſibility. The quick feeling of that ſoft vaſcular ſubſtance which ariſes from bones after exfoliation, and alſo from cartilages, is a convincing proof, that theſe parts, tho' in a natural ſtate the moſt inſenſible of any in the whole body, are not deſtitute of nerves: And it ought always to be remembered, that altho' the ſenſibility of the ſeveral parts proceeds from their nerves alone, yet, in certain parts, theſe nerves may be ſo compreſſed or otherwiſe changed, as either to feel very obtuſely, or perhaps not at all.

(2.) We cannot conclude with certainty, that a part has no nerves beſtowed on it becauſe they are inviſible; for there are many veſſels in the human body whoſe exiſtence we muſt acknowledge, tho' they be too ſmall to be diſcovered by our ſenſes; and it will hardly be doubted, that the ſmaller inſects are furniſhed with muſcular fibres, whereby they perform their various motions, altho' theſe inſtruments are too minute

minute to be diſcovered even by the help of the microſcope.

WITH regard to the tendons, ſince it cannot be denied, that many parts, which were truly muſcular in a *fœtus*, become in an advanced age tendinous, we muſt conclude, that the tendons, as well as the muſcles, are furniſhed with nerves, altho' the nerves of the former may be ſo compreſſed by their hard compacted ſubſtance, as in a great meaſure to be deprived of their ſenſibility. The inflammation and pain conſequent upon opening the *capſulæ* of the joints is a proof that theſe parts have alſo nerves beſtowed upon them: And granting that no nerves could be demonſtrated to terminate in the *pleura* and *dura mater*, as *M. de Haller* alledges, which, however, is doubtful, ſince the accurate *Winſlow* deſcribes them; yet the inflammations with which thoſe membranes are often affected, and that ſenſible granulated ſubſtance which ſometimes ariſes from them, are a ſufficient proof, that they are not without nerves.

ALTHO' anatomiſts are unable to de-

monſtrate the nerves of the marrow and the *cornea*, or conjunctive covering it, yet their ſenſibility is a proof that they are furniſhed with nervous filaments. *M. de Haller*, who ſuppoſes the *cornea* to be a continuation of the *epidermis*, or of the nature of the nails, aſcribes the pain felt upon touching the forepart of the eye, to ſome ſmall nerves that run between the conjunctive and *cornea* *. But not to mention, that the vaſcular ſtructure of theſe membranes ſhew them to be eſſentially different from either the cuticle † or the nails, I may venture to affirm, that no anatomiſt has yet diſcovered any branches of nerves running between the conjunctive and *cornea*; where, indeed, they could be of no uſe, but, if they happened to paſs oppoſite to the pupil, would diſturb the diſtinct viſion of objects. The exiſtence, however, of ſuch nerves, altho' it were granted, would by no means account for the conjunctive

---

* *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 59. and 108.

† Since the *cornea* is covered by the conjunctive, it is not eaſy to conceive how it can be a continuation of the cuticle, which is the moſt external tegument of the body.

junctive being affected by the ſlighteſt touch, and by *ſtimuli* which raiſe no pain in the lips or *fauces*, becauſe it is the extremities of the nerves that feel moſt acutely, and not their ſides, which are defended by the coats which ſurround their medullary ſubſtance. Further, if the pain occaſioned by touching the eye, were owing, not to the conjunctive which covers it, but to the nerves running between it and the *cornea*, how ſhould it happen, that the ſlighteſt touch gives it ſenſible pain, whereas, when the eye-lids are ſhut, a conſiderable preſſure on the forepart of the eye gives little or no uneaſineſs?

LASTLY, Since the ſmall arteries are allowed by *M. de Haller* to have nerves *, which indeed their muſcular ſtructure ſuppoſes, it will follow that the tendons, ligaments, and membranes cannot be altogether deſtitute of nerves; for thoſe parts are, in a great meaſure, a contexture of veſſels that carry either red blood or finer fluids; and no anatomiſt, however ſubtile, can be certain, that the inviſible nervous filaments, entering

---

* *Memoires ſur les parties ſenſibles*, &c. tom. iv, p. 87.

entering thoſe parts along with their arteries or veins, may not be partly beſtowed on that portion of their ſubſtance which is not vaſcular. But be this as it will, if the tendons, ligaments, and membranes conſiſt partly of veſſels that are endowed with nerves, and are not altogether inſenſible, they may certainly, when inflamed, acquire a greater degree of ſenſibility.

PART II.

Of Irritability.

HAVING formerly ſhewn that we have no reaſon to doubt of the irritability of the ſmall arteries and ſeveral other parts, altho' *M. de Haller*'s experiments do not diſcover any ſuch power in them, I ſhall, paſſing by ſome points of leſs moment, proceed to inquire particularly, whether the power of contraction obſervable in ſuch muſcles as are irritated depends upon the nerves, and is connected with ſenſi-

ſenſibility, or whether it may not be a property of that glutinous matter which partly compoſes the muſcular fibres.

In an Eſſay on the vital and other involuntary motions of animals, publiſhed in 1751, I endeavoured to prove that the motions excited in the muſcles by irritation, are owing to a diſagreeable feeling in them or their nerves; but *M. de Häller*, after having made a variety of experiments on living animals, concludes, that irritability is an innate property of the muſcular fibres, or rather of their glutinous ſubſtance, and denies that it depends on the nerves, or has any connection with ſenſibility *. 1. Becauſe the moſt ſenſible parts, ſuch as the ſkin and nerves, are not irritable †. 2. Becauſe the irritability of the muſcles is not obſerved to be in proportion to their ſenſibility ‡; and, 3. Becauſe parts deſtitute of feeling are irritable ‖. After giving a particular anſwer to each of theſe arguments **, I endeavoured to ſhew, by many examples,

---

* Vid. Act. Gottingenſ. vol. II. publiſhed in 1753.

† Act. Gotting. vol. II. p. 134.

‡ Ibid. p. 136. ‖ Ibid. p. 134.

** Vid. Phyſiolog. Eſſays, edit. 1. p. 151—188.

examples, That the irritability of the muſcles or muſcular organs is always increaſed, when, from any cauſe, their ſenſibility is rendered greater-----That in different ages, the irritability of the muſcles bears ſome proportion to their ſenſibility---That when the ſtomach is diſordered, the heart is often rendered more irritable, merely from ſympathy, and in a *nephritis* the ſtomach becomes ſo ſenſible that it can ſcarcely bear any *ſtimulus*. I have proved, by various inſtances, that the irritability of the muſcles is leſſened, or even deſtroyed by cold and by *opium*, which alſo leſſen or deſtroy ſenſibility. I have mentioned ſome deciſive experiments on dying animals, which demonſtrate the connection between ſenſibility and irritability; and, laſtly, I have ſhewn that the *phænomena* of many of the involuntary motions in men, are not to be accounted for, without ſuppoſing them to proceed from feeling *.

Now, in what manner has *M. de Haller* anſwered this chain of argument, which I had brought to prove the connection between irritability and ſenſibility? He tells us,

---

* Vid. Phyſiolog. Eſſays, edit. 1. p. 188.—223.

us, that the nerves, although the moſt ſenſible parts of the body, are not irritable, and that ſenſibility is as the number and bareneſs of the nerves, while irritability is as the number of muſcular fibres expoſed to the irritating cauſe; and therefore theſe two powers are in no proportion to each other. He obſerves, that inſects, which have neither a brain nor nerves, are nevertheleſs endowed with irritability. He mentions a few detached *phænomena*, ſome of which may at firſt view ſeem to make for him, while others are no ways applicable to the point in queſtion; and inſiſts, as he had formerly done, that ſince the muſcles continue to be irritable for ſome time after their nerves are tied or cut, this property certainly ſubſiſts after the power of the nerves is altogether deſtroyed; and therefore can have no dependence on ſenſibility *.

As this laſt argument is the only one of any conſequence which *M. de Haller* has brought againſt the connection which I have endeavoured to prove between irritability

---

* Vid. *Memoires ſur les parties ſenſibles et irritables*, tom. iv. p. 92. & 93. & p. 118—124.

bility and ſenſibility; I ſhall begin with conſidering it particularly: And if I ſhall be able to make it evident by the plaineſt facts, and the faireſt analogy, that the motions of muſcles whoſe nerves are cut, neither ſhew that irritability is independent of the nerves, nor even that it has no connection with ſenſibility, I ſhall find little difficulty in refuting every thing elſe that my learned adverſary has advanced, either in favour of his own ſyſtem, or in oppoſition to mine.

I. Emetics produce convulſive contractions of the ſtomach, not by irritating its muſcular fibres, but by affecting its nerves with a diſagreeable ſenſation. The *ſemen* does not excite the *acceleratores urinæ* into alternate motions by acting on their fibres, but by ſtimulating the nerves of the *urethra*. The ſame is true of the natural *ſtimuli* which act on the inteſtines, heart, and other muſcular organs; nor can one inſtance be produced, in the human body, of any vital or involuntary motion that is owing to an irritation of the muſcular fibres of the

organ

organ itſelf. If then, in a natural ſtate, every inſtance of the motion of muſcles from irritation is owing to a *ſtimulus* acting on their nerves, and not on their fibres, is it not reaſonable to conclude, that the contractile power or irritability of the muſcles depends upon their nerves, and is connected with ſenſibility? But to this concluſion it will be objected, that, in many animals, the muſcles continue to preſerve their power of motion for a conſiderable time after their nerves are tied or cut.

AFTER ſeparating one of the thighs of a frog from its body, the muſcles, when laid bare and irritated, are not brought into any ſtrong contraction; but their fibres, or ſometimes only a few of them, are agitated with a weak tremulous motion. But when the extremity of the crural nerve, where it was cut, is irritated, the muſcles of the thigh and leg are ſtrongly convulſed; and after this part of the nerve has been rendered inſenſible by repeated irritations, and loſt its power over the muſcles, if it be pricked a little below, the member is convulſed as before: And in this manner, by

proceeding gradually downwards with the irritation of the nerve, convulſive motions of the muſcles were renewed and continued for above a quarter of an hour by Dr *Oeder of Coppenhagen* *.

Agreeably to this, when the phrenic nerve, in a dog, is compreſſed or cut, and pricked below this, the diaphragm is contracted in the ſame manner as if the communication of its nerve with the brain had not been interrupted; and by deſcending downwards with the irritation, the motion of the diaphragm may be renewed and continued for ſome time †.

Do not theſe experiments clearly prove, that the nerves, after being ſeparated from the brain and ſpinal marrow, retain their power over the muſcles: And if, after deſtroying, by frequent irritations, the ſuperior part of any nerve, the muſcle to which it belongs can be ſtill convulſed by proceeding downwards with the irritation, till we arrive at the muſcle itſelf, upon whoſe fibres a *ſtimulus* has much leſs effect than

---

* *Memoires ſur les parties ſenſibles*, &c. tom. 2. p. 61.

† *Memoires ſur les parties ſenſibles*, tom. 1. exp. 222. & 225.

than it had on any part of the nerve; does it not clearly follow, that the irritability of the muſcles depends on their nerves; that when the fibres of any muſcle are ſtimulated, the contraction which follows does not proceed from any active power, or innate property in theſe fibres themſelves, but from the irritation of the ſmall nervous filaments which terminate in them; and that a *ſtimulus* applied to a muſcle after its ſeparation from the body produces only a very feeble contraction, while an irritation of its nerve occaſions ſtrong convulſions; becauſe in the former caſe its nerves do not ſuffer ſo much as in the latter, nor are ſo many of them expoſed to the ſtimulating cauſe?

If greater motions were excited in the muſcles by an irritation of their fibres, than of the nerves going to them, or if the muſcular fibres continued to be affected by *ſtimuli*, after their nerves had loſt their power altogether, it might fairly be concluded, that their irritability was in a great meaſure independent of the nerves, and owing to the fibres themſelves; but we ſee

that the contrary of all this is true *: And indeed, whatever deprives the nerves wholly of their powers, renders the muſcles, at the ſame time, altogether incapable of motion. Thus tho' frogs live and can move their limbs and body for ſeveral hours after their head is cut off, yet in leſs than an hour, after injecting a ſolution of *opium* in water into the ſtomach and inteſtines of theſe animals, their muſcles are neither convulſed by the ſtrongeſt *ſtimuli* applied to their fibres or nerves, nor even by an irritation of the ſpinal marrow †.

The plaineſt facts, therefore, prove that the irritability of the muſcles depends on their nerves, and that the nerves preſerve their power over the muſcles for a conſiderable time after they are ſeparated from the brain or ſpinal marrow; and it is really matter of wonder, that theſe points ſhould have ever admitted of diſpute, after the

---

* I have obſerved in frogs, to whom I had give *opium*, that a probe puſhed into the ſpinal marrow produced a feeble contraction of the fore-legs, after pricking and cutting their muſcles had failed to excite any motion.

† Eſſay on the vital motions, &c. p. 372.

the numerous experiments that have been lately made to diſcover the nature and ſource of that power which puts the muſcles in motion *.

---

* "*Pour exciter du movement dans les muſcles par "l'irritation des nerfs, il n'eſt pas neceſſaire, que "ce nerf ait conſervé ſa continuité avec le cerveau, ni a"vec la moëlle de l'epine. Car l'irritation d'un nerf "entierement ſeparé de la moëlle de l'epine ou du cer"veau, produit les mêmes contractions dans le muſcle; "que celle d'un nerf, dont la continuité avec ces parties "eſt conſervée.*" *Memoires ſur la naturé irritable*, tom. i. p. 237. & 238.

"*Ces experiences confirment ce que j'ai dit un peu "plus haut* (p. 238.), *Qu'on comprime, qu'on lie, qu'on "coupe le nerf d'un muſcle, et qu'on intercepte tout le "commerce qu'il avoit avec le cerveau : qu'on irrite ce "nerf, pourvu qu'il ſoit encore frais et humide, ces ir"ritations produeront dans les muſcles auquel ce nerf "aboutit les mêmes mouvemens, qu'elles auroient pro"duit, ſi ſa continuité avec le cerveau etoient entiere. "Ce theoreme ayant été prouvé pour les nerfs, qui obeiſſent "a la volonte, l'eſt ici pour les nerfs vitaux.*" Ibid. p. 245. & 246.

Could any one have imagined that *M. de Haller*, after the above acknowledgment; would have concluded that the motions of thoſe muſcles, which he irritated after he had cut their nerves, were performed without the aſſiſtance of theſe nerves (*a*)? and that he could

(*a*) *Memoires ſur la nature irritable*, *tom.* i. exper. 246.

The nerves are not to be conſidered merely as the excretory ducts of the brain and ſpinal marrow, but as real continuations or productions of their medullary ſubſtance, which are endowed with certain powers that they retain in a great meaſure, even after being divided from their origin: And if the ſeveral parts of a *polypus* preſerve a power of motion and life, and become capable of forming new animals after they are divided, why may not the *medulla oblongata*, ſpinal marrow, and nerves of

have been ſo blinded as not to ſee, that irritability, or the ſecond force which he aſcribes to the muſcles, is owing to the ſmall nerves which terminate in them, and which, like their larger branches, preſerve their power for a conſiderable time after they are cut or tied? *M. de Haller* allows, that the more violent motions of the muſcles are produced by irritating their nerves, and that *opium* which deſtroys ſenſibility deprives the nerves of this power (*a*). Is it not, then, highly probable that the weaker convulſions of the muſcles excited by an irritation of their fibres, which are of a ſimilar nature with thoſe that are ſtronger (*b*), and are alſo deſtroyed by *opium* (*c*), proceed from the power of thoſe ſmall inviſible nervous filaments with which they are furniſhed, and not from any power or property in the muſcular fibres independent of their nerves?

(*a*) Ibid. p. 237. & 256. (*b*) Ibid. p. 256. & 257.

(*c*) Vid. Eſſay on vital motions, *&c.* p. 372. *&c.*

of the larger animals, retain their powers, in a leſſer degree, for a ſhort time after they are ſeparated from each other?

II. Having made it ſufficiently evident that irritability depends on the nerves, we come next to conſider whether the motions excited by *ſtimuli* in thoſe muſcles whoſe nerves are tied or cut, be a proof that irritability has no connection with ſenſibility *.

And here it is obvious to obſerve, that, if the convulſive motions occaſioned by irritating the medullary part of the brain, or the *medulla oblongata* in dying animals, are allowed to be a proof of their ſenſibility; and the like motions excited by ſtimulating the *medulla ſpinalis*, after the head is cut off, ſhew that it ſtill continues to feel †; it will neceſſarily follow, that the convulſions

---

* *Vid. Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 119.

† That the convulſive motions excited in the muſcles by irritating the *medulla oblongata* or *ſpinalis* are owing, not to the mechanical propulſion of any fluid through the nerves proceeding from them, but to the ſenſibility of thoſe parts, can ſcarce be doubted, ſince the convulſions occaſioned by irritating and breaking down

convulſions produced in the muſcles by irritating their nerves after they are cut, or below the ligature after they are tied, are equally ſtrong indications of ſenſibility in theſe nerves. Nay, *M. de Haller* himſelf has expreſsly told us, however unfavourable it may be to his ſyſtem, that after cutting the ſpinal marrow of frogs in two, their hind-legs continued to be ſenſible, and the animals felt the irritation of their nerves. See *Memoires ſur la nature ſenſible*, &c. tom. 1. exp. 201. & 202.

But further, unleſs thoſe motions produced by irritation were owing to a diſagreeable ſenſation excited in the muſcles or their nerves, how comes it that the muſcles of a frog's legs and thighs, which are not

---

down the brain in a dog to whom Dr *Kaau Boerhaave* had given ſix grains of *opium* were a hundred times leſs than thoſe which he had been in uſe to obſerve in other dogs who had got nothing to lull their ſenſes (*a*). Further, the effects produced by irritating the *medulla oblongata* or *ſpinalis* are vaſtly greater than could be expected from ſuch a cauſe; thus not only a ſmall irritation with the point of a needle, but acrid ſubſtances, which do not act by mechanical impulſe, ſo affect thoſe parts as to excite ſtrong contractions of the muſcles.

(*a*) Impet. faciens *Hippocrat.* dict. No. 435.

not ſenſibly moved by pricking its feet or toes immediately upon decollation, ſhould, in ten or fifteen minutes after this, be brought into motion by touching thoſe parts with one's finger, and be ſtrongly convulſed by cutting them with a pen-knife? In this caſe, does not the greater pain occaſioned by cutting off the head, render the irritation of the feet ineffectual, at firſt, to produce any motion in the muſcles of the legs and thighs? and are not theſe parts moved, afterwards, by a much weaker *ſtimulus*; becauſe the pain of decollation is ſo much abated, as not to prevent the animal from feeling when its toes are touched? This experiment, which I have repeated ſeveral times, is related more fully in the firſt edition of the preceeding obſervations; and as I looked upon it to be a deciſive proof of the connection between ſenſibility and irritability, I deſired that the learned *M. de Haller*, or any of his diſciples, would favour me with ſome probable explication of it upon their principles. But as my learned adverſary, in his anſwer, has not taken the ſmalleſt notice of this experiment,

ment, nor attempted, in any manner, to remove its force, I muſt be allowed to think it now an unanſwerable proof, that the motions produced by irritation depend upon the ſenſibility of the parts.

It is obſervable, that an irritation of the nerves, which are the moſt ſenſible parts, produces the moſt violent motions in the muſcles; and when they are, by being ſtretched, rendered more ſuſceptible of pain, ſtill greater convulſions are occaſioned by pricking them: The irritation of the muſcular fibres, which are leſs ſenſible than the nerves, excites only a weaker contraction; and *ſtimuli* applied to the tendons, which are in a natural ſtate deſtitute of any painful feeling, produce no motion at all, altho' theſe tendons be really a continuation of the muſcular fibres. Theſe *phænomena* will not be eaſily explained, except upon the ſuppoſition, that the motions excited by irritation are owing to an uneaſy feeling.

But this will appear ſtill more evident, if we conſider that thoſe motions which are occaſioned by *ſtimuli*, acting, not on the organs

organs moved, but on diſtant parts, cannot poſſibly be owing to any change made on the glutinous matter of the muſcles, but proceed from that *ſympathy* which prevails in the nervous ſyſtem; and muſt be aſcribed to an uneaſy ſenſation in the part irritated, ſince all *conſent* ſuppoſes feeling, and is indeed inexplicable upon any other principle.

Thus the contraction of the pupil from light affecting the *retina;* the convulſive motions of the ſtomach and diaphragm, which attend an inflammation of the kidneys, or are occaſioned by a ſtone irritating the ureters; the accelerated motion or palpitation of the heart from a diſorder in the ſtomach; the convulſion of the diaphragm from an irritation of the noſe, and its continued contraction when the *inteſtinum rectum* or bladder are ſtimulated.

These, and other ſympathetic motions, which might be mentioned, are all owing to an uneaſy feeling in the parts irritated, whence certain organs, with which they have a particular ſympathy by means of the nerves, are either excited into convul-

ſive contractions, or rendered ſo irritable as to be much more eaſily affected by *ſtimuli* than uſual. If, then, motions occaſioned by the irritation of a diſtant part, proceed from feeling, it will appear no leſs probable that thoſe contractions, which are excited in any muſcular organ by irritating its fibres or nerves, are owing to their being hurt by the *ſtimulus*.

It is obſervable, that all ſympathetic motions ceaſe as ſoon as the brain and ſpinal marrow are deſtroyed, or the communication between them and the nerves is cut off; the reaſon of which is, that the nerves have no ſympathy but at their origin *: while, on the other hand, the contractions produced by an immediate irritation of the parts moved, or of their nerves, continue for ſome time, becauſe the nerves do not at once loſe their powers after being ſeparated from the brain and ſpinal marrow.

---

* The truth of this propoſition, and that ſympathy is not owing to any communication or connection of the nerves in their courſe from the brain to the ſeveral parts of the body, I have endeavoured to prove in ſome obſervations on the ſympathy of the nerves, which I propoſe ſoon to lay before the public.

IF it ſhall be aſked, How can a part feel when it has no connection with the brain? I anſwer, That I know not how it feels either when it is connected with the brain, or ſeparated from it; but undoubted experiments ſhew that it feels in both caſes *; for we have ſeen that the nerves, when irritated, exhibit the ſame marks of ſenſation after being tied or cut, as they do in an entire ſtate: And here I am naturally led to conſider the metaphyſical arguments, which *M. de Haller* has brought to prove that irritability has no connection with ſenſibility; but ſince, in his late remarks †, he has done little more than to renew the ſame objections to which I had formerly given a ſufficient anſwer ‡, and as a phyſical queſtion muſt be determined by facts and obſervations, and not by metaphyſical reaſoning, I ſhall only ſuggeſt, that when my learned adverſary confines the ſoul to

the

---

* *M. de Haller* himſelf acknowledges that the hinder limbs of a frog feel after its ſpinal marrow is cut in two. *Memoires ſur la nature irritable*, tom. i. exp. 202.

† *Memoires ſur les parties ſenſibles, &c. tom.* iv. *p.* 101. &c.

‡ Vid. Phyſiological eſſays, edit. 1. p. 168. &c.

the brain, he has certainly forgot that a frog lives, moves, and is ſenſible, thirty hours, a viper three days, and a tortoiſe ſix months, after the loſs of the brain *. When he argues, that if the whole nervous ſyſtem, or the whole body be animated, we ought to be ſenſible of loſing a portion of our ſoul when an arm or a leg is cut off †; he does not ſeem to have been aware, that an immaterial ſubſtance cannot, like the body, be divided by the anatomical knife, and that the indiviſibility of the ſoul does not depend on the unity of that body which it animates, but on its own particular nature. Laſtly, When he concludes that a member, whoſe nerves are tied or cut, or which is ſeparated from the body, can have no kind of ſenſibility, becauſe, when it is pricked, or otherwiſe wounded, the perſon, to whom it belongs, feels not the ſmalleſt pain ‡, he had certainly not attended to what I had ſuggeſted to ſhew, that it is only in the brain that the ſoul reaſons, remembers, and is conſcious of the feelings of

---

* Vid. Phyſiological eſſays, edit. 1. p. 168. &c.; and Eſſay on the vital motions, p. 385 and 386.

† *Memoires ſur les parties ſenſibles, &c. tom.* iv. *p.* 121. ‡ Ibid. p. 119. and 120.

of the different parts of the body *. When a nerve or a muſcle is irritated after its communication with the brain is cut off, although there be ſome kind of feeling in the part itſelf, yet the animal has no conſciouſneſs of this, becauſe the common *ſenſorium* is not affected by it.

That learned philoſopher *Calwallader Colden*, Eſquire, Governor of New-York, ſeems to have underſtood this matter well, and has ſet it in ſuch a light as may be ſufficient to anſwer all *M. de Haller*'s metaphyſical difficulties. In a letter to me, *dated at New-York, April* 15. 1760, he expreſſes himſelf as follows:------" The mind is not " confined to any particular part of the " body; for nothing can act where it is " not. But we are only conſcious of thoſe " ſenſations which are communicated by " the nerves to the common *ſenſorium*; for " otherwiſe the action of any one part has " no relation to the whole ſyſtem; we are " not otherwiſe conſcious that WE feel. In " this common reference of every part to " the whole, perſonality or ſameneſs and " conſciouſneſs conſiſt. So likewiſe it ſeems " probable,

---

* Phyſiological eſſays, edit. 1. p. 169. &c.

" probable, that all operations of the mind, " of which we are conſcious, or all actions " of the will, ariſe in the common *ſenſorium.* " The mind may have different perceptions " of the actions of bodies or of matter in " the part on which any body immediately " acts, from what it has of the ſame action " communicated to the common *ſenſorium*; " but we are only conſcious of the laſt: and " ſo we may not be conſcious of the operations of the mind when its action does " not proceed from the common *ſenſorium.*"

BUT to return,

WHETHER the irritability of the muſcles depends on their nerves, and is connected with ſenſibility, or not, is a point that muſt be determined by experiments alone; and for this purpoſe it is ſufficient, if we can prove that the nerves are endowed with feeling and a power of moving the muſcles; and that, after they are tied or cut, they retain theſe powers in ſome degree and for ſome time. It is altogether unneceſſary to inquire, whether thoſe powers of the nerves be owing to the particular diſpoſition

poſition and arrangement of the matter of which they are compoſed, or to ſome immaterial principle animating them. This is a queſtion intirely metaphyſical and no ways connected with the dependence of irritability on the nervous power; and whichſoever way it may be decided, the arguments will remain in full force which I have uſed to prove, that the motions produced in the muſcles by irritation are owing to the nervous power, and connected with ſenſibility.

I have formerly offered it as my opinion, that not only the ſenſibility of the ſeveral parts, but that power of motion which the muſcles poſſeſs, muſt be referred, at laſt, to that living principle which many of the wiſeſt philoſophers in all ages have ſuppoſed to animate the human body: But I will venture to ſay, that, leaving the ſoul out of the queſtion, many who are not *materialiſts*, will think it as reaſonable to aſcribe feeling to the nerves, as to attribute the active power of irritability to the glue which partly compoſes the muſcular fibres; for dead matter ſeems to be as little capable of real activity as of ſenſibility.

III. IT is argued, that the irritable power of the muſcles appears to be independent of the nerves, becauſe many inſects are poſſeſſed of it in a remarkable degree, altho' they have neither a brain nor nerves *.

To this argument I had anſwered before, that even the ſmalleſt animals might have nerves, or ſomething analogous to them; though, on account of their exility, they are not to be diſcovered even by the beſt microſcopes. But now I am told, that, ſince I affirm inſects have nerves, it is incumbent on me to demonſtrate thoſe parts which our ſenſes diſavow †. Are we then to deny the exiſtence of every thing which does not fall under the cognizance of our ſenſes, and reject all reaſoning concerning the ſtructure of animals from analogy? Will *M. de Haller* think it a ſufficient anſwer to the arguments he has brought to prove, that the medullary fibres of the brain and the nerves are hollow tubes and filled with a fluid ‡, to ſay, that, ſince he affirms

---

* *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 92. 122. and 123.

† Ibid. p. 122.

‡ Primæ lineæ phyſiolog. 387. &c.

affirms the nerves to be hollow, it is incumbent on him to demonſtrate theſe cavities which our ſenſes diſavow?

As a ſingle nervous fibre is too ſubtile to be diſtinguiſhed, even in the largeſt animals, by the beſt microſcopes, Why may not inſects be endowed with nerves, altho' too ſmall to be diſcovered by our ſenſes? The ingenious Mr *Lyonet* of the *Hague* has, as I am informed, by the help of the microſcope, diſcovered and delineated not only the *medulla ſpinalis* in the caterpillar that feeds on the willows, but the nerves ſent out from it to the ſeveral parts of that animal; and it is highly probable, that many other inſects who have no head or brain, and have been thought to want nerves, may really be furniſhed with them, altho', on account of their ſmallneſs, they may for-ever eſcape our moſt induſtrious reſearches.

But ſuppoſing, what is by no means probable, that moſt inſects were really deſtitute of nerves; would it thence follow, contrary to the cleareſt experiments, that the muſcles of larger animals do not owe their power of motion, as well as ſenſation,

tion, to their nerves? Becauſe inſects, in which we can obſerve no brain or nerves, are endowed with feeling, and undoubtedly perform voluntary motion, are we to conclude, that ſenſibility and voluntary motion in men do not depend on the brain and nerves?

It is obſervable, that *M. de Haller* only concludes, from the inviſibility of the nerves in *polypi* and other microſcopic inſects, that there may be *muſcular motion* where there are *no nerves* *, but if he had reaſoned fairly, he ſhould have alſo added, *nor muſcles*; for in thoſe inſects we can as little demonſtrate a muſcular as a nervous ſtructure. This, however, was wiſely kept out of view, as the abſurdity of aſcribing muſcular motion to animals deſtitute of muſcular fibres, would have been rather too glaring, and muſt have expoſed the weakneſs and fallacy of this whole argument.

The truth of the matter is, that many inſects, in which we can neither diſcover muſcles nor nerves, appear, as far as we can

---

* *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 123.

can judge from experiments and obſervation, to be endowed not only with a power of motion, but alſo with ſenſation; the proper concluſion from which is, not that the muſcles and nerves in men and other animals are unneceſſary to motion and ſenſation; but that thoſe inſects are either poſſeſſed of theſe inſtruments, altho', on account of their ſmallneſs, they be inviſible, or that the great AUTHOR of nature has endowed almoſt every part of their bodies with the powers of ſenſation and motion, although he has neither given them nerves nor muſcular fibres like thoſe of the larger animals.

IV. IN order to ſhew the connection between irritability and ſenſibility, I had obſerved that the irritability of the muſcles or muſcular organs in the human body, was obſerved to bear a proportion to their ſenſibility *. To this it has been anſwered, that ſenſibility is proportioned to the number and bareneſs of the nerves, while irritability is in proportion to the number of fibres expoſed to the irritating cauſe.

---

* Phyſiological eſſays, edit. 1. p. 189.

cauſe *. The obſervation is moſt certainly true; but, like many others that have been brought againſt me, is nothing to the purpoſe. The nerves and the ſkin, altho' the moſt ſenſible parts of the body, are not irritable like the muſcles, becauſe they are not, by their ſtructure, fitted for motion; the number, therefore, and bareneſs of the nerves beſtowed on any part, will not make it irritable, unleſs it be of a muſcular nature: And ſurely no one, who had any tolerable knowledge of the human body, could ever imagine, that the moving power of its different parts ſhould be proportionable to their ſenſibility *alone*, whatever difference there might be in their ſtructure or other circumſtances. But where the ſame muſcular ſtructure takes place, and the ſame number of fibres are affected by the ſtimulating cauſe, there irritability will be found always proportional to ſenſibility. The effects of inflammation and other examples formerly produced †, clearly prove, that the irritability of the muſcles

---

* *Vid. Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 92.

† Vid. Phyſiological eſſays, edit. 1. p. 189. &c.

ſcles and organs, in the ſame perſon, is at different times, *cæteris paribus*, proportioned to their ſenſibility: And the frequent palpitations of the heart, from ſlight cauſes, in thoſe women who are poſſeſſed of a very delicate nervous ſyſtem, as well as many other facts that might be mentioned, ſhew that the irritability of the heart in different perſons is proportional to its ſenſibility. In like manner, are not the *inteſtinum rectum* and the bladder of urine rendered impatient almoſt of any *ſtimulus*, when their ſenſibility is increaſed by their being inflamed, excoriated, or ulcerated?

BUT it has been ſaid, that the ſtomach, tho' leſs irritable, is nevertheleſs more ſenſible than the heart *. The nerves on the internal ſurface of the ſtomach have a very peculiar feeling, whereby they are diſagreeably affected by many ſubſtances that neither offend the tongue, *fauces*, nor even the eyes: but, on the other hand, they can bear the touch of brandy, vinegar, and hot ſpiceries, with much leſs uneaſineſs than thoſe parts, or than the ſkin when deprived of the cuticle. The ſenſibility of the nerves

---

* Act. Gotting. vol. II. p. 136.

nerves on the inſide of the ſtomach is, therefore, of a peculiar kind, and cannot be properly compared with the feeling of the nerves in the other organs. But ſuppoſing, that the heart were leſs ſenſible than the internal ſurface of the ſtomach, yet it is to be conſidered, that theſe organs are not, in other reſpects, alike; and there can be no doubt, that of organs, which are only in part muſcular, the moſt ſenſible may be the leaſt irritable. The muſcular fibres in the ſtomach are vaſtly fewer in number, and may, for any thing we know, be alſo leſs ſenſible than thoſe of the heart. The ſmall degree of ſenſibility which the muſcular fibres of the ſtomach poſſeſs, when compared with the delicate and peculiar feeling of its internal ſurface, ſeems to be the reaſon why, in living animals whoſe *abdomen* is opened, pricking the ſtomach with a needle or the point of a knife does not produce any ſuch convulſive motions of its muſcular coat, as are occaſioned by the much weaker irritation of its inner ſurface by emetics, diſagreeable aliments, and other ſlight cauſes.

THE

The muſcular fibres, as far as can be gathered from experiments on brutes, or obſervations on men, are only endowed with a middle kind of ſenſibility, and have a leſs acute feeling than the nerves, or even than the ſkin and ſeveral other parts [*]. It is noways inconſiſtent, therefore, with the great irritability of the heart, that it may be leſs ſenſible than the ſkin when deprived of the *cuticle*, or even than the internal ſurface of the ſtomach, or that animals do not ſhew ſigns of great pain when it is irritated [†]. All that is required to its greater irritability, is, that it abound more in muſcular fibres, and be poſſeſſed of a ſenſibility, at leaſt equal, if not ſuperior to that of the other muſcles. The very numerous muſcular fibres of the heart will not be denied, and there is no experiment or obſervation that ſhews theſe fibres, in a natural ſtate, to be leſs ſenſible than thoſe of the

---

[*] Vid. Brockleſby in Philoſoph. Tranſact. vol. xlix. p. 243.

[†] After opening the *thorax*, it is not to be expected that animals can feel additional pain from wounding their heart, unleſs it were much more ſenſible than the parts which have been cut before. Beſide, we cannot judge with certainty of the feelings of a dying animal.

other muſcles *. Indeed the heart's preſerving life ſo long, and its being ſo ſenſible of all kind of *ſtimuli*, are ſtrong preſumptions of its delicate feeling, and that its nerves, as well as thoſe of the inteſtines may, perhaps, be ſo conſtituted as to preſerve their powers longer, after being ſeparated from the brain and ſpinal marrow, than thoſe of the other muſcles.

It has been juſtly obſerved by *M. de Haller*, that the heart, in dying animals, is much more affected by the gentle *ſtimulus* of warm water puſhed into its ventricles, than by applying the moſt acrid liquors to its external ſurface, or even pricking it with the point of a knife †: And we know that, in living animals, the motion of the heart is often greatly increaſed by a degree of acrimony in the blood imperceptible to the ſenſes, and by the chyle which is much leſs acrid than thoſe aliments from which it

---

* The external ſurface of the heart and inteſtines is rendered leſs ſenſible than it would otherwiſe be, by being covered with membranes that are poſſeſſed of very little feeling: and hence that woman, whoſe inteſtines were handled by *Peyerus*, complained of no pain. Pareg. anat. exercitat. i. cap. iv.

† Act. Gotting. vol. I.

it is prepared, and which the ſtomach and inteſtines bear without any pain, or remarkable increaſe of their periſtaltic motion.

But if the irritability of the heart neither depends on its ſenſibility nor on its nerves, for what purpoſe has nature beſtowed any upon it? Since its motion is only of the involuntary kind, one would be apt to think it might have been better without nerves altogether; for, upon *M. de Haller*'s ſyſtem of irritability, they ſeem to be of little other uſe *, than to make the heart

---

* Vid. *Seconde lettre de Mr Caldani; Memoires ſur les parties ſenſibles*, tom. iii. &c. p. 371.

Mr Caldani affirms, that the irritation of the *par vagum* and intercoſtals, after opening the *thorax*, neither accelerated nor renewed the contraction of the heart in lambs. But this authority will be more than balanced by that of *Willis* and *Lower*, *Kaau Boerhaave* and *Laghi*. *Kaau Boerhaave* tells us, that, after the heart's motion had become languid in dogs, whoſe *cranium* he had opened, he always obſerved, that it contracted more briſkly when the *cerebellum* or *medulla oblongata* was irritated. *Laghi* ſays the ſame thing happened when he ſtimulated the cardiac nerves. And *Willis* and *Lower* have long informed us, that when the *par vagum* and intercoſtals were tied, the heart

heart liable, when diſeaſed, to a painful ſenſation, from which it muſt have been exempted, had it been deſtitute of nerves.

V. It is ſaid that irritability cannot depend on ſenſibility, becauſe the greateſt pains do not always occaſion convulſive motions, and violent convulſions are often without pain. With the ſame view we are told that cauſtic vapours deſtroy the irritability of the heart without exciting pain, and that paralytic members retain their feeling after their power of motion is loſt. See *Memoires ſur les parties ſenſibles et irritables*, *tom.* iv. *p.* 92. & 93.

In anſwer to theſe objections, it may be ſufficient to obſerve, that,

(*a*) Although in the gout, rheumatiſm, toothach, and other diſeaſes, there is often violent pain without any convulſions, becauſe

---

heart was affected with uncommon tremors and palpitations. Indeed, when numerous experiments ſhew that the other muſcles are convulſed by irritating their nerves, it muſt appear highly improbable, that the nerves of the heart have no power over it. However, we ſhall afterwards ſee, from the effects of *opium*, that the irritability of the heart depends upon the nervous power, as well as that of the other muſcles.

becauſe the parts affected are not muſcular, or perhaps have no particular ſympathy with any of the muſcles, it can never be concluded, contrary to the evidence of numberleſs experiments and obſervations, that a violent pain in the muſcles or their nerves does not produce convulſions. Beſides the experience of every practical phyſician will atteſt, that acute pain is moſt apt to occaſion convulſive motions in thoſe patients whoſe nerves are endowed with the moſt delicate feeling.

(*b*) IN women, convulſions are often owing to ſudden and ſtrong affections of the mind, or to an uneaſy ſenſation in the ſtomach or inteſtines. But as theſe cauſes produce convulſive motions by diſagreeably affecting the nervous ſyſtem, nothing can be concluded from them againſt the connection between irritability and ſenſibility; on the contrary, they are rather a proof, that the former of theſe powers depends on the latter.

FURTHER, as thoſe general convulſions which often happen to hyſteric patients from a diſordered ſtate of the alimentary canal, can only be accounted for from the

ſympathy that obtains in the nervous ſyſtem, they cannot be independent of feeling, without which there could, ſtrictly ſpeaking, be no conſent of parts which are diſtant from each other.

The whole body is often more diſordered from a diſagreeable ſenſation in certain nerves or organs, than from acute pain: Thus a *nauſea* or ſickneſs at ſtomach, particular ſmells and ſounds, will affect ſome very delicate women with fainting and convulſions; and more violent motions are occaſioned by tickling the ſoles of the feet, or the ſides, than by cutting them with a ſharp inſtrument: But from theſe examples it can never be juſtly inferred, that there is no connection between irritability and ſenſibility.

(*c*) Not only cauſtic vapours, but foul air, the ſteams of ſulphur and of ſtrongly fermenting liquors, quickly deſtroy animals, and put a ſtop to the motion of the heart; but, in the preſent argument, no concluſion can be drawn from this effect of thoſe ſubſtances. *M. Caldani*, who is quoted by *M. de Haller*, argues, that irritability

bility cannot depend on ſenſibility, becauſe water or air makes the heart contract more briſkly than cauſtic vapours. As a proof of this, he tells us, that when frogs were incloſed in a glaſs-receiver full of theſe vapours, the heart ſoon beat feebly, or not at all, and could not be made to move briſkly by any irritation: He adds that, ſince theſe cauſtic vapours applied to ſenſible parts occaſion pain, it follows, that irritability is deſtroyed by thoſe things which excite ſenſibility *. Such indefinite experiments and ill-founded concluſions ſcarce deſerve any anſwer.

Caustic vapours, whether they excite pain or not, deſtroy the motion of the heart by their corroſive acrimony or poiſonous quality; and the heart of a frog, immerſed in boiling water or oil of vitriol, ceaſes immediately to move, becauſe its nerves and fibres are greatly injured by them. Although, therefore, moderate *ſtimuli* excite and increaſe the motion of the heart, very hot, acrid, and corroſive ſubſtances

---

* See *Memoires ſur les parties ſenſibles*, &c. tom. iii. p. 369. et 370.

ſtances render it ſoon callous and incapable either of feeling or motion.

(*d*) WHEN a palſied leg or arm retains its ſenſibility, we can only conclude that voluntary motion may be loſt, while the ſenſe of feeling remains: For the muſcles of members that are not only paralytic, but even withered in a good degree, may be excited into convulſive motions by *ſtimuli*, and therefore are not wholly deprived of their irritability. Of this I had a ſtrong inſtance ſome time ago in a man, whoſe left arm had been not only palſied for twelve years, but was much extenuated, notwithſtanding which, the muſcles of this arm were brought into contraction every time they received the electrical ſhock. But further, it ought to be obſerved, that, in paralytic caſes, the nerves which go to the muſcles may be often more obſtructed or weakened, than thoſe which are beſtowed on the ſkin.

VI. As *opium* is well known to weaken or deſtroy the power of feeling in animals, I thought it might throw no ſmall light

on the controverſy concerning irritability, to determine by experiments, how far that ſubſtance alſo weakened or deſtroyed the moving power of the muſcles.

THESE experiments, which are to be found in my Eſſay on the vital motions of animals *, and in the Edinburgh Phyſical eſſays †, were made with care, and are related with preciſion; nor have I ever truſted any one fact to a ſingle experiment.

WHEN one would determine accurately the effects of *opium* on the motion of the heart, it is not ſufficient to inform us, as ſome have done, that, after giving *opium* to dogs or frogs, the animals were opened, and their heart was obſerved to continue its motion, altho' the inteſtines had loſt theirs. Experiments made and narrated in this looſe uncircumſtantiate manner are by no means deciſive. The method I followed was, to open and inſpect the animals, which were expoſed to the action of *opium*, at different times, and to obſerve accurately the ſteps by which it firſt weakened, and at laſt put a ſtop to the motion of the heart: and

---

* Pag. 370.——376.

† Vol. 2. art. xx. p. 280.——316.

and in order to aſcertain exactly the number of its pulſations, I always uſed a watch with a hand that marked ſeconds.

From about twenty experiments conducted in this manner, I have clearly, and, as I have been told, to the ſatisfaction of ſeveral able phyſicians, proved, That *opium*, injected into the ſtomach and inteſtines, or applied to the bare abdominal muſcles of frogs, ſoon leſſens, and, after ſome time, entirely deſtroys all feeling and power of motion, not only in the parts to which it is applied, but through the whole body: That *opium* produces theſe effects by its action on the ends of the nerves which it touches [*]: That a ſolution of *opium* in water, conveyed into the ſtomach and inteſtines of frogs, ſoon renders the motion of the heart remarkably ſlow, and at length puts a ſtop to it altogether [†]: That *opium* weakens and deſtroys the motion of the heart, in frogs, much ſooner than the deſtruction

---

[*] See anſwer to object. 9. below.

[†] In half an hour it reduced the pulſations of the heart in one frog, from above 60 to 17 in a minute: And in another frog, after an hour and ſix minutes, the heart did not move above ſeven times in a minute. See Edinburgh Phyſical Eſſays, vol. 2. p. 281. and 282.

ſtruction of the brain and ſpinal marrow: That it operates much more ſlowly in deſtroying the heart's motion in frogs deprived of their brain and ſpinal marrow, than it does when theſe animals are entire; and that, when applied to the bared abdominal muſcles of the former, it ſeems to put a ſtop to the motion of the heart only a very little ſooner than would happen from the loſs of the brain and ſpinal marrow alone *. Whence it follows, that *opium* weakens or deſtroys the moving power of the heart chiefly, if not wholly, by the mediation of the brain and ſpinal marrow, and conſequently that the motion of the heart depends upon the influence of the nerves which it receives from thoſe parts.

---

* The heart of a frog, thirty-five minutes after it was deprived of its brain and ſpinal marrow, beat thirty times in a minute. The heart of another frog, whoſe brain and ſpinal marrow were deſtroyed, beat 26 times in a minute, after a ſolution of *opium* in water had been applied thirty-ſix minutes to its bared abdominal muſcles: But after a ſolution of *opium* had been applied in the ſame manner, and for the ſame time, to an entire frog, its heart only beat ſix timesin a minute. *Vid.* Edinburgh Phyſical eſſays, vol 2. art. xx. No. 5. 6. and 7.

parts. The deſtruction of the brain and ſpinal marrow only prevents the derivation of any new *influence* from theſe parts to the heart, but does not immediately deſtroy the power of the cardiac nerves themſelves. On the other hand, *opium*, applied in ſufficient quantity to the ſenſible parts of frogs, not only ſoon puts a ſtop to the action of the brain and ſpinal marrow, and thus produces the ſame effect upon the heart as does the loſs of thoſe parts, but alſo deſtroys the power of every nervous filament proceeding from them, and therefore puts a ſtop to the motion of the heart in frogs, ſooner than the deſtruction of the brain and ſpinal marrow.

I have ſhewn likewiſe, from the obſervations of Dr *Kaau Boerhaave* *, and from an experiment made at my deſire by Dr *Ramſay*, that opium, introduced into the ſtomach, or injected into the cavity of the *abdomen*, ſoon renders the pulſe remarkably ſlower in dogs: And it is well known that men who have ſwallowed *laudanum* by miſtake, in ſo great quantity as to occaſion death, have had a ſlower pulſe and reſpiration

---

* Impet. faciens Hippocrat. dict. §. 434. 435. & 436.

ration than patients who die either of common diſeaſes, or by other poiſons.

To my experiments made on animals with *opium*, and the concluſions drawn from them, *M. de Haller* has made the following objections, which I have been at pains to collect and range in ſome kind of order, ſubjoining to each what, it is hoped, will be thought a ſufficient anſwer.

Objection I. OPIUM heats the body, produces ſweat, and quickens the pulſe, inſtead of making it ſlower, as Dr *Whytt* has alledged *.

Anſwer. ALTHO' *Opium* in a moderate doſe, generally heats the body a little, promotes ſweat, occaſions thirſt, and renders the pulſe ſomewhat fuller; yet I am not ſo certain of its effect in accelerating the motion of the heart. But ſuppoſing *opium* in a ſmall quantity always quickened the pulſe, it would not thence follow, contrary to the cleareſt experiments on brutes, that it muſt have the ſame effect when taken ſo liberally as to endanger life; for we know, that

---

* *Mem. ſur les parties ſenſibles*, &c. tom. iv. p. 128.

that notwithſtanding it gives ſpirits and even vigour to many people who are accuſtomed to it, yet if uſed, even by them, in exceſs, it produces a ſtupor and a paralytic debility for ſome time *. *Opium*, in a moderate doſe, may, by its ſtimulating quality, heat the body and quicken the pulſe a little; while a greater quantity of it may ſo weaken the ſenſibility and active power of the whole nervous ſyſtem, as, notwithſtanding its *ſtimulus*, to render the motion of the heart more languid. Agreeably to this, Dr *Langriſh* has obſerved, that an ounce of laurel-water given to a dog occaſioned much ſtronger convulſions than five or ſix ounces. The firſt quantity was ſufficient to irritate the nervous ſyſtem, the laſt quickly deſtroyed the powers of life altogether, and therefore prevented the laurel-water from producing, by its *ſtimulus*, any conſiderable convulſive motions †.

Bᴜᴛ, whatever may be the effect of *opium*,

---

* *Kaau* Boerhaave Impet. faciens Hippocrat. dict. § 438.

† See Langriſh's Phyſical experiments on brutes, p. 67.

um, in a ſmall doſe, upon men, nothing is more certain, than that it renders the pulſe remarkably ſlower in frogs and other animals *. My very ingenious colleague Dr *Alexander Monro junior*, who has been lately employed in making many curious experiments with *opium* on frogs, laid bare the heart of one of theſe animals, and then poured thirty drops of a filtrated ſolution of *opium* in water into the cavity of the *abdomen*. In two or three minutes after this, the heart did not beat above half its uſual number of times in a minute: And in four minutes its pulſations were reduced almoſt to one third of their uſual number. In five or ſix minutes the blood ceaſed to move in the ſmall veſſels of the foot; altho', upwards of four hours after the ſolution of *opium* was dropped into the cavity of the *abdomen*, the heart was obſerved to perform about a dozen of very feeble contractions.

In a frog, which I immerſed in a ſolution of *opium*, after laying open its whole *abdomen* and *thorax*, the motion of the heart was, in eleven minutes, reduced to leſs than one

---

* Vid. note, p. 262. and 263. above.

fifth of its uſual quickneſs, and in about twenty minutes it ceaſed altogether *.

Object. 2. THE diminution of the quickneſs of the pulſe in that dog into whoſe *abdomen* Dr *Ramſay* injected a ſolution of *opium*, was owing to the opening of the *thorax*, and drawing aſunder the ribs: And Dr *Whytt* certainly exaggerates matters greatly, when he repreſents this dog's pulſe as beating 150 times in a minute, becauſe this is too great a number to be counted †.

Anſwer. M. DE HALLER has forgot, that, before either the ribs were hurt, or the *thorax* laid open, the pulſe of this dog was reduced from 150 to 76 in a minute ‡. The effect of the *opium* was ſo ſudden in this experiment, that four minutes after it was injected into the *abdomen*, the dog was rendered ſo inſenſible, that he felt no pain when the teguments of his *thorax* were diſſected.

WITH regard to the number of this dog's

---

* Edinburgh Phyſ. eſſays, vol. 2. art. xx. No 11.

† *Memoires ſur les part. ſenſib.* &c. tom. iv. p. 126.

‡ See Edinburgh Phyſical eſſays, vol. 2. p. 298. and 299.

dog's pulſe before the injection of the *opium*, I know Dr *Ramſay*'s accuracy too well to ſuſpect that he could be ſo far miſtaken, as any ways to affect the concluſion to be drawn from this experiment: And if Dr *Langriſh* obſerved the pulſe to beat 84 times in a minute, in a dog 22 inches high *, it cannot appear altogether improbable that, in Dr *Ramſay*'s dog, who was not ſeven inches high, and only ſix months old, the velocity of the pulſe might be 150 †; for altho'

---

* Phyſical experiments on brutes, p. 95.

† It may be proper to obſerve, that this dog, having had ſeveral experiments made on him before, ſeemed to be greatly frightened when Dr *Ramſay* laid hold on him; and that on this account his pulſe might, perhaps, beat twenty times more in a minute than if the animal had been quite free from fear. This circumſtance, however, which I was only lately informed of by Dr *Ramſay*, cannot invalidate the concluſion which I have drawn from his experiment; becauſe this dog's pulſe, four minutes after the ſolution of *opium* was injected into the *abdomen*, only beat 76 times in a minute.

But in order to remove all reaſonable doubt concerning this matter, Dr *Ramſay* has taken the trouble to make the following experiment, which I ſhall relate in his own words.

altho' the heart of a full grown man does not commonly contract above 70 times in a minute, yet in a child of half a year old, it makes nearly 130 vibrations in that time.

When *M. de Haller* ſays a pulſe of 150 in a minute is too quick to be numbered, he ſhews himſelf not to have attended with any accuracy to the velocity of the pulſe in ſeveral diſeaſes. Thus in the fever preceeding

---

"The experiment I made this afternoon to "determine the quickneſs of the pulſe in a ſmall dog, "was as follows.

"The dog was ſeven weeks old, and weighed exactly twenty ounces; his pulſe beat from 15 to 18 in "five ſeconds, which, at the loweſt, makes 180 pulſations in a minute; and that I might be under no "miſtake, I examined both the pulſation of his heart "through the ribs and the beating of the arteries of his "legs. I continued the examination a quarter of an "hour, during which I felt the pulſations both of the "heart and arteries about twenty times. As I gave the "dog no pain, and as he ſeemed fond of me, I conclude the above to be his natural pulſe; and if I can "be certain of any thing, I have made no miſtake in "this laſt experiment. It is obvious to obſerve that "the quicker motion of the heart in this dog, than in "that one into whoſe *abdomen* I afterwards injected a "ſolution of *opium*, muſt have been owing to his being "much younger and of a ſmaller ſize."

ceeding the eruption of the ſmall-pox, in children from two to four years, I have moſt commonly obſerved the pulſe to beat 14 or 15 times in five ſeconds, *i. e.* from 166 to 180 in a minute. In a peripneumonic fever, in children of two or three years, I have met with a pulſe which beat 16 or 17 times in five ſeconds. In ſuch patients as died of a dropſy in the ventricles of the brain, I have generally, a day or two before their death, obſerved the pulſe to beat from 150 to near 200; and a phyſician of this place aſſured me, that in a patient of his who died of this diſeaſe, the pulſe moved at the rate of 216 times in a minute.

Object. 3. M. DE HALLER could never kill a dog with *opium*, in whatever doſe he gave it; and therefore this ſubſtance does not deſtroy the moving power or irritability of the heart *.

Anſwer. IT is certain that Dr Mead † and others have killed dogs with *opium*; and practical

---

* *Mem. ſur les parties ſenſibles*, &c. *tom.* iv. *p.* 126.

† Treatiſe on poiſons, Eſſay iv.

practical phyſicians have had too frequent experience of its proving fatal to the human ſpecies, when taken in an immoderate doſe [*]: Frogs and many other animals are likewiſe deſtroyed by it. But ſuppoſing that *opium*, even in large quantities, did not kill dogs; yet, if in a few minutes it renders their pulſe remarkably ſlower than uſual, it cannot be denied that it leſſens the irritability of their heart, while it deſtroys, at leaſt for a time, the ſenſibility and moving power of all the other parts of the body.

Object. 4. SINCE *opium* which deſtroys feeling, does not put a ſtop to the motion of the heart, the irritability of this organ cannot depend on its ſenſiblility [†].

Anſwer. I have ſhewn from various experiments [‡], which are confirmed by thoſe of *Kaau Boerhaave* [§] and *Alſton* [‖], that *opium* ſoon

---

[*] *Hiſtoire Acad. Royale des ſciences*, 1735, p. 6.; Eſſay on the vital motions, p. 194.; and Edinburgh Med. Eſſays, vol. 5. part. 1. art. xii. ſect. iii.

[†] *Memoires ſur les parties ſenſibles, tom.* iv. &c. *p.* 128.

[‡] Edinburgh Phyſical eſſays, vol. 2. art. xx.

[§] Impet. faciens Hippocrat dict. §. 434. 435. & 436.

[‖] Edinburgh Medical eſſays, vol. v. part. 2. art. xii.

ſoon renders the motion of the heart remarkably ſlow in dogs and frogs, and at laſt puts a final ſtop to it. Whence it appears that *opium*, which leſſens or deſtroys the power of feeling in animals, alſo leſſens or deſtroys the motion of the heart, as well as that of the other organs. *M. de Haller*'s experiments, from which he has concluded that *opium* does not affect the moving power of the heart, really prove no ſuch thing; they only inform us that three frogs were opened after having ſwallowed *opium*, and that the heart continued to move after the motion of the inteſtines had ceaſed *. But as it is not ſaid, how long the *opium* had remained in the ſtomach of theſe animals before they were opened, nor what proportion the heart's motion bore to its uſual quickneſs, we can draw no certain concluſion from theſe experiments, except that *opium* received into the ſtomach

---

* *Memoires ſur la nature irritable, tom.* 1. *exp.* 528. 529. 531. Since *M. de Haller* has added, that when the motion of the heart failed, it was renewed by *ſtimuli*, it is not to be doubted that the *opium* had affected it conſiderably; becauſe the heart of a frog, which has got no *opium*, continues to beat for many hours after its *thorax* is laid open.

ſtomach of frogs, deſtroys the motion of the inteſtines ſooner than that of the heart. In my experiments, altho' *opium* forced into the ſtomach and inteſtines of frogs intirely deſtroyed the power of the muſcles of voluntary motion in about half an hour, yet the heart continued to move a conſiderable time after this, but with great ſlowneſs and ſeeming languor *. This organ, therefore, is not exempted from the power of *opium*, altho' it is not ſo ſoon affected by it as the other muſcles.

SINCE it is acknowledged on all hands, that *opium* deſtroys the irritability of the inteſtines and other muſcles, as well as their ſenſibility; its affecting the heart more ſlowly muſt be owing, either to the *opium* not being applied ſo near to it †, or to this organ

---

* See Eſſay on the vital motions, p. 371. and 372.; and Edinb. Phyſical eſſays, vol. 2. p. 281. and 282.

† When a frog, whoſe *thorax* and *abdomen* were opened, was laid in a ſolution of *opium* in water, the voluntary muſcles were deprived of their irritability in ten or twelve minutes, and the heart's motion ceaſed in twenty minutes. Whereas, when the ſolution of *opium* was injected into the ſtomach and inteſtines of another

organ being endowed with a ſtronger power of motion; for it is by no means probable, that the irritability of the heart depends on a cauſe quite different from that of all the other muſcles.

Object.

another frog, its heart continued after an hour and ſeven minutes to move at the rate of ſeven times in a minute, *i. e.* about eight or nine times ſlower than it does in a natural ſtate. *Vid.* Edin. Phyſ. eſſays, vol. 2. p. 282. and 291.

But that *opium,* when applied to the heart itſelf, deſtroys its power of motion as ſoon as that of the other muſcles, appears from the following experiment of Dr *Al. Monro junior.* The Doctor having laid bare the heart, and then injected into a large vein which runs along the under and middle part of the *abdomen* on the outer ſide of the *peritonæum* in frogs, a few drops of a ſolution of *opium* in water, *viz.* twelve drops into two frogs, and ſix drops into a third; he obſerved, that as ſoon as the *ſolution* had reached the heart, that organ was rendered incapable of expelling its contents, and, in leſs than one minute after this, became ſo entirely paralytic as not to make the leaſt contraction, on the ſtrongeſt irritation, whether applied to its outer or inner ſurface.

This experiment of Dr *Monro,* while it confirms what I had advanced concerning the effect of *opium* in deſtroying the moving power of the heart, is intirely ſubverſive both of *M. de Haller*'s experiments, and of the concluſions he has drawn from them.

Object. 5. Dr Whytt, by ſhewing that the motion of the heart in frogs continues after the deſtruction of the brain and ſpinal marrow, has proved againſt himſelf that the irritability of the heart is equally independent of the nerves and of ſenſibility *.

Anſwer. The motions produced by irritating the ſpinal marrow ſhew that it retains its power over the muſcles after the loſs of the brain: And the convulſions produced by irritating the crural nerve of a frog after it is ſeparated from the body, prove that the nerves continue to preſerve their power of putting the muſcles in motion, after their communication with the brain and ſpinal marrow is quite cut off. Wherefore the continuance of the motion of the heart in frogs, is no proof that the moving power of this muſcle does not depend on its nerves, and conſequently on its ſenſibility †.

Further,

---

* *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 130.

† See above, p. 228—246. where this point is more fully diſcuſſed.

Further, that the motion of the heart is not independent on the brain and ſpinal marrow, evidently appears from ſome experiments lately made by Dr *Al. Monro junior* who has obſerved, that as ſoon as theſe parts are deſtroyed, the moving power of the heart becomes ſo weak, that it is unable to carry on the circulation in the veſſels of the hind-legs; notwithſtanding that, immediately after the deſtruction of the brain and ſpinal marrow, the motion of the heart becomes, for a little time, quicker than uſual.

It is obſervable, that as thoſe animals, whoſe blood is almoſt as cold as the *medium* ſurrounding them, live a conſiderable time after the loſs of their heart, ſo their nerves preſerve their powers much longer, after the deſtruction of the brain and ſpinal marrow, than the nerves in men and other animals, whoſe life ceaſes when the circulation of the blood is ſtopt, and whoſe nerves ſoon loſe all their powers, after they are ſeparated from their origin.

Object. 6. When *opium* is given in an

immoderate doſe, ſo as to kill animals, it muſt like other poiſons weaken all the powers of life; and therefore muſt render the pulſe ſlower before death [*].

Anſwer. ALTHOUGH every poiſon and every diſeaſe, which prove mortal, deſtroy the moving power of the heart at laſt; yet moſt poiſons and diſeaſes render the pulſe quicker in men than it is naturally, till juſt about the time of death, or a very little before it, when the patient becomes in a greater meaſure inſenſible, and all the powers of life begin to ceaſe; and *M. de Haller* himſelf has remarked that poiſons almoſt always increaſe the periſtaltic motion of the inteſtines in animals [†]. A horſe who was killed in three days by repeated doſes of laurel-water, a little before his death, had a pulſe which beat at the rate of 103 in a minute, *i. e.* three times quicker than it had done when the animal was in health [‡]. A dog 22 inches high, by

---

[*] *Memoires ſur les parties ſenſibles*, tom. iv. p. 128. and 129.

[†] *Memoires ſur la nature irritable*, tom. i. p. 339.

[‡] *Langriſh*'s Phyſical experiments, p. 72.

by ſwallowing, for above four months, the powder of green laurel leaves, had his pulſe, before he died, raiſed from 84, its natural quickneſs, to 157 ſtrokes in a minute *. *Opium*, on the contrary, given in a great doſe, ſoon renders the pulſe much ſlower in dogs and frogs; and this ſlowneſs gradually increaſes till the motion of the heart ceaſes altogether.

BUT the following experiments which I made lately, will demonſtrate beyond doubt that *opium* renders the motion of the heart ſlower than other poiſons.

(*a*) HAVING laid open the *thorax* and *abdomen* of a frog, I poured into the *abdomen* a tea ſpoonful of water in which I had diſſolved two grains of *opium*. In a minute and a half, the pulſations of this animal's heart were reduced from 54 to 26 in a minute. After five minutes the heart beat only 18, and after twenty minutes only ten times in a minute.

AN hour after the ſolution of *opium* was applied to the *viſcera* of the *abdomen*, the heart made only between ſix and ſeven very

---

* *Langriſh*'s Phyſical experiments, p. 95. and 103.

very feeble pulſations in a minute, and in leſs than 15 minutes after this its motion ceaſed altogether.

(*b*) Having laid open another frog, I poured into its *abdomen* a tea ſpoonful of water, in which I had diſſolved three grains of corroſive mercury. In five minutes, the pulſations of the heart were reduced from 54 to 46 in a minute. After ten minutes the heart beat 38, and after twenty minutes 18 times in a minute; but its contractions were extremely feeble.

In leſs than half an hour from the application of the ſolution of the corroſive mercury, the motion of the heart ceaſed, and the animal ſeemed to be quite dead.

(*c*) I laid open a third frog, and poured into its *abdomen* a tea ſpoonful of malt ſpirits nearly of the ſame ſtrength with common French brandy. In a minute and a half, the pulſations of the heart were reduced from 54 to 52 in a minute *. After

---

* As I had formerly obſerved the hearts of frogs to beat about 60 times in a minute, when their *thorax* was opened, I ſuſpect its ſlower motion in the above three

ter five minutes the heart beat 50, and after eleven minutes 41 times in a minute. Fifty minutes after the malt ſpirits were applied to the *viſcera* of the *abdomen*, the auricle and heart made 34 very feeble contractions in a minute. In ten minutes more, the animal was become altogether inſenſible, and made no motion when its muſcles were pricked or torn; but the auricle continued to beat 33 times in a minute, and the heart made nearly the ſame number of extremely feeble and incomplete contractions.

It was obſervable, that the heart of this frog was much paler than that of the frog, to which the *opium* was applied, nor was there any blood collected in its auricle. See *Eſſay on the vital motions*, p. 371. and 372.; alſo *Edinburgh Phyſical eſſays*, vol. ii. p. 282. 286. 290. and 313.

WHETHER an apoplexy occaſioned by

three frogs might be owing to their having been kept in the houſe five days before making theſe experiments: Perhaps alſo the hearts of frogs beat ſlower towards the end of Auguſt than in June or July, when my former experiments were moſtly made.

ſpirit of wine kills animals without quickening their pulſe before death, as *M. de Haller* ſeems to inſinuate [*], I ſhall not ſay; but I know certainly, that in other apoplexies the pulſe, though at firſt ſlow, becomes, almoſt always, conſiderably quick before the patient's death. I have ſeen it riſe from 60 or 65 to above 150 beats in a minute.

I have obſerved the heart of a frog moving 20 times in a minute, near two hours after the deſtruction of its brain and ſpinal marrow; whereas, in little more than an hour after injecting *opium* into the ſtomach and inteſtines of another frog, the heart beat only ſeven times in a minute [†]. When *opium* is given in ſuch quantity to frogs as not to kill them, their pulſe is rendered by it remarkably ſlow for many hours; but afterwards, when the effects of this poiſon begin to abate, the pulſe gradually returns to its natural quickneſs, the inteſtines recover their periſtaltic motion [‡], and the

---

[*] *Memoires ſur les parties ſenſibles*, *tom.* iv. *p.* 129.

[†] Edinburgh Phyſical eſſays, vol. ii. p. 282. 283. and 284.

[‡] Vid. *Memoires ſur la nature irritable*, tom. i. exp. 456.

the other parts of the body regain their ſenſibility and moving power.

Upon the whole, we may conclude, that the remarkably ſlow pulſe of thoſe animals which are killed by *opium*, is owing to the peculiar virtue which this ſubſtance has of weakening or deſtroying the moving power of the heart, as well as the ſenſibility and irritability of all the other parts of the body.

But this matter is put beyond all doubt, by two experiments Dr *Bard* of *New-York* made upon himſelf, which he has publiſhed in his Theſis, ſummer 1765.

At ſeven in the morning he took 1½ grains of *opium*, his pulſe then beat 71 in a minute, at eight it beat 69; at 8¼ it beat 67; at 8½ it beat 66; at 8¾ it beat 64; at 9 it beat 64; at 9½; after breakfaſt, it beat 66; at 10 it beat 65; at 10½ it beat 61; at 11 it beat 60; at 11½ it beat 59; at 12 it beat 57; and this was the loweſt to which his pulſe ever fell.

He made another experiment by taking 1½ grains of *opium*, which he divided into ſix parts: At 7½ in the morning he took

one of theſe parts, and his pulſe was 70; at 8 he took another, his pulſe 70; at $8\frac{1}{3}$ he took a third, his pulſe 70; at $9\frac{1}{2}$, after breakfaſt, he took a fourth, and his pulſe was 75; at 10 he took a fifth, and his pulſe 76; at $10\frac{1}{2}$ he took the ſixth, his pulſe was 71; at 11 it was 66; at $11\frac{1}{2}$ it was 64; at 12 it was 64; at $12\frac{1}{2}$ it was 63; at 1 it was 63; at 2 it was 62; and this was the loweſt to which his pulſe ever fell.

Object. 7. OPIUM may, perhaps, by its viſcoſity alone, put a ſtop to the motion of the heart mechanically *.

Anſwer. THAT a ſcruple of *opium*, which has been known to kill a full grown woman †, ſhould render the whole maſs of blood ſo viſcid, as to put a ſtop mechanically to the motion of the heart, is a ſuppoſition that needs not be refuted. What becomes of this mortal viſcoſity of *opium*, when it is ſwallowed daily to the quantity of two or three, nay even ten drachms, by thoſe who have been long accuſtomed

---

* *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 129. and 130.

† Edinburgh Medical eſſays, vol. v. art. xii. ſect. iii.

to it? Although the nerves, by long uſe, may ſuffer leſs from the action of *opium*, yet no cuſtom could prevent the blood from being rendered proportionally more viſcid by it.

Object. 8. To aſſert that *opium* kills frogs more quickly when they are intire, than when they are deprived of the brain and ſpinal marrow, is a paradox, which would ſcarcely be rendered probable by the greateſt number of experiments*.

Anſwer. MANY things ſeemingly improbable have been proved to be true. Thoſe experiments which I have related were made with care; and no one has yet pretended to ſay that he has repeated them, and found the event different. But if *M. de Haller* had allowed himſelf to conſider this matter more coolly, he would have ſeen no inexplicable paradox here; for if *opium* produces its effects by acting on the extremities of the nerves which it touches, and if the nerves have no communication or ſympathy, but through the mediation of

---

* *Memoires ſur les parties ſenſibles*, &c. tom. iv p. 129.

of the brain and ſpinal marrow, it muſt neceſſarily follow, that *opium* applied to the abdominal muſcles of a frog deprived of the brain and ſpinal marrow, can have little or no influence on its heart *; and therefore will not kill it ſo ſoon as if the animal had been entire.

Object. 9. Dr Whytt's experiments with *opium* on frogs, have been proved to be *fundamentally* erroneous by *Fontana*, who has ſhewn that *opium* applied externally, and its ſolution in water applied to the nerves, does not, in any degree, deſtroy that

---

* In this caſe the *opium* can, in no other way, affect the heart or its nerves, except in ſo far as ſome of its finer parts are abſorbed by the bibulous veins of the abdominal muſcles, and carried along with the blood to that organ: (*a*) And as Dr *Al. Monro junior* has found by experiments that the moving power of the heart and action of the veſſels is ſo weakened, in frogs, by the deſtruction of the brain and ſpinal marrow, that the circulation in many of the ſmall veſſels either ceaſes altogether, or goes on very languidly, it muſt follow, that the finer parts of the *opium* will be very ſlowly abſorbed, and conſequently can have but little influence in deſtroying that power of motion which remains in the heart after the brain and ſpinal marrow are deſtroyed.

(*a*) See Edinburgh Phyſical eſſays, vol. 2. p. 303. and 304.

that power whereby they make even the muſcles of voluntary motion contract *.

Anſwer. M. FONTANA's experiments are not more contradictory to mine, than to thoſe of *M. de Haller*, which ſhew that *opium* deſtroys the ſenſibility of the nerves, and the irritability of all the muſcles except the heart †. The truth is, that when they are *fairly* repreſented, they are not *directly* repugnant to either. *M. Fontana* does not ſay, that *opium*, applied externally to frogs, has no effect in deſtroying the power of the nerves ‡. He only tells us, that a ſolution

---

* *Memoires ſur les parties ſenſibles*, &c. *tom.* iv. p. 130. and 131.

† *Vid. Memoires ſur les parties ſenſibles*, tom. I. p. 237. & 339. *Opium ſupprime cette faculté des nerfs par laquelle ils excitent du mouvement dans les muſcles.*

‡ There is nothing more certain than that a ſolution of *opium* in water, applied externally, kills frogs; I ſhall therefore content myſelf with mentioning the following experiment, which is altogether deciſive of this matter. Dr *Al. Monro junior*, having applied to the belly and hind legs of a frog ſome lint moiſtened with about a hundred drops of a ſolution of *opium* in water; in leſs than three hours, the animal was convulſed and unable to move its body out of the place where

lution of *opium* in water applied to the bared trunk of the crural nerves of frogs did not, in any degree, deſtroy their power *; but he does not alledge that *opium* applied to the ſtomach, inteſtines, or other parts where the nerves terminate, has no ſuch effect. *M. Fontana*'s experiments, therefore, prove nothing, except that *opium* does not deſtroy the power of the nerves, when it is applied to their trunks, in the ſame manner that it does, when it touches their extremities; a fact that will ſcarcely be doubted of by any one who conſiders, that in the former caſe, the medullary ſubſtance of the nerves is defended by their coats which are in a great meaſure inſenſible †.

ALTHO'

---

where it lay, and after five hours was quite dead; for although the lint was removed and the ſkin waſhed, it did not recover.

* *Memoires ſur les parties ſenſibles*, &c. *tom.* iii. *p.* 211.

† *M. Fontana*, found that although ſpirit of wine deſtroyed the power of the nerves in that part which it touched, yet, when theſe nerves were irritated a little below this, the muſcles contracted as uſual (*a*). The ſame thing is true of a ligature and of a red hot iron, oil of vitrol, or other acrid ſubſtances, whoſe action is totally different from that of *opium*, which affects not only the nerves it touches, but the whole ſyſtem.

(*a*) *Memoires ſur les parties ſenſibles*, *tom.* iii. *p.* 466.

ALTHOUGH *M. Fontana*'s experiments are neither of that nature, nor related with that preciſion, which would be neceſſary to determine the operation of *opium*, yet he has inferred from them, that *opium* does not produce its effects on animals, except it be introduced into the blood; and that, when it is carried to the muſcles, it changes the nature of their fibres, or of their glue, which, he thinks, is probably the ſeat of irritability. This concluſion is not only contrary to what the beſt writers have taught concerning the action of *opium* *, but is utterly inconſiſtent with ſeveral experiments, that are altogether deciſive as to this matter, and which *M. Fontana* ſeems to have been unacquainted with. Thus the paralytic weakneſs brought on a dog's

---

* See *Jones*'s Myſteries of *opium*.; *Mead*'s Treatiſe on poiſons; *Alſton* in the *Edinburgh* Medical eſſays, vol. v. part. 1. article xii.; *Kaau Boerhaave Impet faciens Hippocrat. dict.* § 434.—438.; and the Baron *Van Swieten*'s Comment. in *Boerhaav.* Aphor. 229. No. 2. More authors might be mentioned; but theſe are fully ſufficient to balance the authority of *Tralles*, who, though greatly extolled by *M. de Haller*, ſeems to have erred not a little in his account of the action of *opium* upon animals.

hinder legs, in a minute after injecting a ſolution of *opium* into his inteſtines, and almoſt inſtantaneouſly after it was thrown into the cavity of the *abdomen*, cannot poſſibly be owing to the finer parts of the *opium* entering the blood, and changing the nature of the muſcular glue. Further, frogs, which live above two hours and a half after being deprived of their heart, are killed in half an hour after the loſs of that organ, by injecting a ſolution of *opium* in water into their ſtomach and inteſtines *; although in this caſe, the *opium* could neither enter the blood nor be carried by it to the muſcles; while, on the other hand, *opium* has very little effect in haſtening the death of thoſe frogs which have been deprived of their brain and ſpinal marrow.

As far as we can judge from experiments, *opium* acts either on the extremities of the nerves to which it is firſt applied, in which way it ſeems to produce its ſpeedieſt effects, or by being carried, by the abſorbent veins, into the blood, where it affects the nerves of the whole vaſcular ſyſtem. But

---

* Edinburgh Phyſical eſſays, vol, ii. p. 281.; and Eſſay on the vital motions, p. 37.

But as the effects of *opium* generally ceaſe, in a great meaſure, in men, by the time it may be ſuppoſed to have got all out of the ſtomach and inteſtines, and to have made its way into the blood, we may conclude, that the chief action of this ſubſtance in *them*, is on the nerves of the ſtomach and inteſtines, to which it is firſt applied.

VII. I am here naturally led to conſider *M. de Haller*'s notion of irritability as an active property of the glutinous matter which partly compoſes the muſcles; but as he has advanced nothing new in ſupport of this doctrine *, I ſhall only obſerve, that ſince it has been proved that the moving power of the muſcles depends upon their nerves, we are under no neceſſity of aſcribing this power either to their glutinous or earthy part. The tendons and ligaments, which abound much more in glue than the muſcles, are not irritable; and the greater irritability of young animals, is doubtleſs owing to their greater ſenſibility, which is alſo the cauſe of their being

---

* *Vid. Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 123 and 124.

ſo remarkably affected by *opium.* Women of the moſt delicate frame have often thinner blood and leſs, or at leaſt not more glue in their ſolid parts, than ſtrong men whoſe muſcles are not liable to be affected with convulſions from ſlight cauſes; and it is demonſtrable, that thoſe convulſive motions which proceed from the irritation of a diſtant part, are owing ſolely to the ſympathy of the nerves.

Strange certainly it is, to find men of learning and abilities, miſled by a few ill-underſtood experiments, and truſting to metaphyſical notions, uſing every art to prove, that the irritable power of the muſcles does not depend on their nerves, and aſcribing it to a ſubſtance the moſt unlikely to be poſſeſſed of it; a ſubſtance which, devoid of active powers, appears remarkably inert, and is not endowed even with elaſticity in ſo remarkable a degree as glaſs, fine wool, hair, and many other ſubſtances *.

AND

---

* When *M. de Haller* ſays that even the glue of animals ſeparated from them, ſhews as it were the remains of irritability, becauſe it contracts itſelf after being drawn

AND thus I have endeavoured to ſhew that *M. de Haller* has equally failed in the ſupport of his own theory, and in the arguments he has brought to confute what I had advanced concerning the nature of irritability. Nor can I help thinking it a ſtrong preſumption againſt my learned adverſary's opinion, that ſo able a writer has argued ſo inconcluſively in its defence; for to expreſs myſelf in the words of the Poet,

> -------- *Si Pergama dextra*
> *Defendi poſſent, etiam hac defenſa fuiſſent.*

I ſhall only add, that if there ſhould be found a few *phænomena* which, at firſt ſight, do not appear ſo favourable to the doctrine of irritability as depending upon the nerves, and being connected with ſenſibility, this would be far from being a ſufficient reaſon for rejecting an opinion, which

drawn out (*a*), he miſtakes elaſticity for the remains of irritability; and has forgot that the glue got by boiling the ligaments and ſkin is ſtronger, and poſſeſſed of this elaſtic power in as great, or a greater degree, than that which the muſcles afford.

(*a*) *Memoires ſur les parties ſenſibles*, &c. tom. iv. p. 124.

is ſupported by ſuch a chain of argument, and agrees ſo well with the various *phænomena* of the animal frame. There are few even of the beſt founded theories in natural philoſophy or phyſic, which are not liable to difficulties and objections, that, ſometimes, cannot be eaſily removed. At preſent, the greateſt philoſophers have only acceſs, as it were, to the ſurface of things, without being able to penetrate into many of the myſteries of nature.

THE END.